SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

CONTINUITY.[1] II

THE so-called non-Newtonian mechanics, with mass and shape a function of velocity, is an immediate consequence of the electrical theory of matter. The dependence of inertia and shape on speed is a genuine discovery and, I believe, a physical fact. The principle of relativity would reduce it to a conventional fiction. It would seek to replace this real change in matter by imaginary changes in time. But surely we must admit that space and time are essentially unchangeable: they are not at the disposal even of mathematicians; though it is true that Pope Gregory, or a daylight-saving bill, can play with our units, can turn the third of October in any one year into the fourteenth, or can make the sun south sometimes at eleven o'clock, sometimes at twelve.[2]

But the changes of dimension and mass due to velocity are not conventions, but realities; so I urge, on the basis of the electrical theory of matter. The Fitzgerald-Lorentz hypothesis I have an affection for. I was present at its birth. Indeed I assisted at its birth; for it was in my study at 21 Waverley Road, Liverpool, with Fitzgerald in an arm chair, and while I was enlarging on the difficulty of reconciling

[1] Address of the president of the British Association for the Advancement of Science, Birmingham, 1913.

[2] In the historical case of governmental interference with the calendar, no wonder the populace rebelled. Surely some one might have explained to the authorities that dropping leap year for the greater part of a century would do all that was wanted, and that the horrible inconvenience of upsetting all engagements and shortening a single year by eleven days could be avoided.

the then new Michelson experiment with the theory of astronomical aberration and with other known facts, that he made his brilliant surmise: "Perhaps the stone slab was affected by the motion." I rejoined that it was a 45° shear that was needed. To which he replied, "Well, that's all right—a simple distortion." And very soon he said, "And I believe it occurs, and that the Michelson experiment demonstrates it." A shortening long-ways, or a lengthening cross-ways would do what was wanted.

And is such a hypothesis gratuitous? Not at all: in the light of the electrical theory of matter such an effect ought to occur. The amount required by the experiment, and given by the theory, is equivalent to a shrinkage of the earth's diameter by rather less than three inches, in the line of its orbital motion through the ether of space. An oblate spheroid with the proper excentricity has all the simple geometrical properties of a stationary sphere; the excentricity depends in a definite way on speed, and becomes considerable as the velocity of light is approached.

All this Professors Lorentz and Larmor very soon after, and quite independently, perceived; though this is only one of the minor achievements in the electrical theory of matter which we owe to our distinguished visitor, Professor H. A. Lorentz.

The key of the position, to my mind, is the nature of cohesion. I regard cohesion as residual chemical affinity, a balance of electrical attraction over repulsion between groups of alternately charged molecules. Lateral electrical attraction is diminished by motion; so is lateral electric repulsion. In cohesion both are active, and they nearly balance. At anything but molecular distance they quite balance, but at molecular distance attraction predominates. It is the diminution of the predominant partner that will be felt. Hence while longitudinal cohesion, or cohesion in the direction of motion, remains unchanged, lateral cohesion is less; so there will be distortion, and a unit cube xyz moving along x with velocity u becomes a parallelopiped with sides $1/k^2$, k, k; where $1/k^2 = 1 - u^2/v^2$.[3]

The electrical theory of matter is a positive achievement, and has positive results. By its aid we make experiments which throw light upon the relation between matter and the ether of space. The principle of relativity, which seeks to replace it, is a principle of negation, a negative proposition, a statement that observation of certain facts can never be made, a denial of any relation between matter and ether, a virtual denial that the ether exists. Whereas if we admit the real changes that go on by reason of rapid motion, a whole field is open for discovery; it is even possible to investigate the changes in shape of an electron—appallingly minute though it is—as it approaches the speed of light; and properties belonging to the ether of space, evasive though it be, can not lag far behind.

Speaking as a physicist I must claim the ether as peculiarly our own domain. The study of molecules we share with the chemist, and matter in its various forms is investigated by all men of science, but a study of the ether of space belongs to physics only. I am not alone in feeling the fascination of this portentous entity. Its curiously elusive and intangible character, combined with its universal and unifying

[3] Different modes of estimating the change give slightly different results; some involve a compression as well as a distortion—in fact the strain associated with the name of Thomas Young; the details are rather complicated and this is not the place to discuss them. A pure shear, of magnitude specified in the text, is simplest, it is in accord with all the experimental facts—including some careful measurements by Bucherer—and I rather expect it to survive.

permeance, its apparently infinite extent, its definite and perfect properties, make the ether the most interesting as it is by far the largest and most fundamental ingredient in the material cosmos.

As Sir J. J. Thomson said at Winnipeg:

> The ether is not a fantastic creation of the speculative philosopher; it is as essential to us as the air we breathe. . . . The study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist.

Matter it is not, but material it is; it belongs to the material universe and is to be investigated by ordinary methods. But to say this is by no means to deny that it may have mental and spiritual functions to subserve in some other order of existence, as matter has in this.

The ether of space is at least the great engine of continuity. It may be much more, for without it there could hardly be a material universe at all. Certainly, however, it is essential to continuity; it is the one all-permeating substance that binds the whole of the particles of matter together. It is the uniting and binding medium without which, if matter could exist at all, it could exist only as chaotic and isolated fragments: and it is the universal medium of communication between worlds and particles. And yet it is possible for people to deny its existence, because it is unrelated to any of our senses, except sight—and to that only in an indirect and not easily recognized fashion.

To illustrate the thorough way in which we may be unable to detect what is around us unless it has some link or bond which enables it to make appeal, let me make another quotation from Sir J. J. Thomson's address at Winnipeg in 1909. He is leading up to the fact that even single atoms, provided they are fully electrified with the proper atomic charge, can be detected by certain delicate instruments—their field of force bringing them within our ken—whereas a whole crowd of unelectrified ones would escape observation.

> The smallest quantity of unelectrified matter ever detected is probably that of neon, one of the inert gases of the atmosphere. Professor Strutt has shown that the amount of neon in 1/20 of a cubic centimeter of the air at ordinary pressures can be detected by the spectroscope; Sir William Ramsay estimates that the neon in the air only amounts to one part of neon in 100,000 parts of air, so that the neon in 1/20 of a cubic centimeter of air would only occupy at atmospheric pressure a volume of half a millionth of a cubic centimeter. When stated in this form the quantity seems exceedingly small, but in this small volume there are about ten million million molecules. Now the population of the earth is estimated at about fifteen hundred millions, so that the smallest number of molecules of neon we can identify is about 7,000 times the population of the earth. In other words, if we had no better test for the existence of a man than we have for that of an unelectrified molecule we should come to the conclusion that the earth is uninhabited.

The parable is a striking one, for on these lines it might legitimately be contended that we have no right to say positively that even space is uninhabited. All we can safely say is that we have no means of detecting the existence of non-planetary immaterial dwellers, and that unless they have some link or bond with the material they must always be physically beyond our ken. We may, therefore, for practical purposes legitimately treat them as nonexistent until such link is discovered, but we should not dogmatize about them. True agnosticism is legitimate, but not the dogmatic and positive and gnostic variety.

For I hold that science is incompetent to make comprehensive denials, even about the ether, and that it goes wrong when it makes the attempt. Science should not deal in negations: it is strong in affirmations, but nothing based on abstraction ought to presume to deny outside its own region. It often happens that things ab-

stracted from and ignored by one branch of science may be taken into consideration by another: Thus, chemists ignore the ether; mathematicians may ignore experimental difficulties; physicists ignore and exclude live things; biologists exclude mind and design; psychologists may ignore human origin and human destiny; folk-lore students and comparative mythologists need not trouble about what modicum of truth there may be in the legends which they are collecting and systematizing, and microscopists may ignore the stars. Yet none of these ignored things should be denied.

Denial is no more infallible than assertion. There are cheap and easy kinds of scepticism, just as there are cheap and easy kinds of dogmatism; in fact, scepticism can become viciously dogmatic, and science has to be as much on its guard against personal predilection in the negative as in the positive direction. An attitude of universal denial may be very superficial.

To doubt everything or to believe everything are two equally convenient solutions; both dispense with the necessity of reflection.

All intellectual processes are based on abstraction. For instance, history must ignore a great multitude of facts in order to treat any intelligently: it selects. So does art; and that is why a drawing is clearer than reality. Science makes a diagram of reality, displaying the works, like a skeleton clock. Anatomists dissect out the nervous system, the blood vessels and the muscles, and depict them separately—there must be discrimination for intellectual grasp—but in life they are all merged and cooperating together; they do not really work separately, though they may be studied separately. A scalpel discriminates: a dagger or a bullet crashes through everything. That is life—or rather death. The laws of nature are a diagrammatic framework, analyzed or abstracted out of the full comprehensiveness of reality.

Hence it is that science has no authority in denials. To deny effectively needs much more comprehensive knowledge than to assert. And abstraction is essentially not comprehensive: one can not have it both ways. Science employs the methods of abstraction and thereby makes its discoveries.

The reason why some physiologists insist so strenuously on the validity and self-sufficiency of the laws of physics and chemistry, and resist the temptation to appeal to unknown causes—even though the guiding influence and spontaneity of living things are occasionally conspicuous as well as inexplicable—is that they are keen to do their proper work; and their proper work is to pursue the laws of ordinary physical energy into the intricacies of "colloidal electrolytic structures of great chemical complexity" and to study its behavior there.

What we have clearly to grasp, on their testimony, is that for all the terrestrial manifestations of life the ordinary physical and chemical processes have to serve. There are not new laws for living matter, and old laws for non-living, the laws are the same; or if ever they differ, the burden of proof rests on him who sustains the difference. The conservation of energy, the laws of chemical combination, the laws of electric currents, of radiation, etc.—all the laws of chemistry and physics—may be applied without hesitation in the organic domain. Whether they are sufficient is open to question, but as far as they go they are necessary; and it is the business of the physiologist to seek out and demonstrate the action of those laws in every vital action.

This is clearly recognized by the leaders, and in the definition of physiology by

Burdon Sanderson he definitely limited it to the study of "ascertainable characters of a chemical and physical type." In his address to the Subsection of Anatomy and Physiology at York in 1881 he spoke as follows:

> It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of "vitalism." Before that time, even the greatest biologists—*e. g.*, J. Müller—recognized that the knowledge biologists possessed both of vital and physical phenomena was insufficient to refer both to a common measure. The method, therefore, was to study the processes of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. The most efficient cause [conducing to the change] was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the conservation of energy. . . .
>
> Investigators who are now working with such earnestness in all parts of the world for the advance of physiology have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness.

Professor Gotch, whose recent loss we have to deplore, puts it more strongly. He says:

> It is essentially unscientific to say that any physiological phenomenon is caused by vital force.

I observe that by some critics I have been called a vitalist, and in a sense I am; but I am not a vitalist if vitalism means an appeal to an undefined "vital force" (an objectionable term I have never thought of using) as against the laws of chemistry and physics. Those laws must be supplemented, but need by no means be superseded. The business of science is to trace out their mode of action everywhere, as far and as fully as possible; and it is a true instinct which resents the medieval practise of freely introducing spiritual and unknown causes into working science. In science an appeal to occult qualities must be illegitimate, and be a barrier to experiment and research generally; as, when anything is called an act of God—and when no more is said. The occurrence is left unexplained. As an ultimate statement such a phrase may be not only true, but universal in its application. But there are always proximate explanations which may be looked for and discovered with patience. So, lightning, earthquakes and other portents are reduced to natural causes. No ultimate explanation is ever attained by science: proximate explanations only. They are what it exists for; and it is the business of scientific men to seek them.

To attribute the rise of sap to vital force would be absurd, it would be giving up the problem and stating nothing at all. The way in which osmosis acts to produce the remarkable and surprising effect is discoverable and has been discovered.

So it is always in science, and its progress began when unknown causes were eliminated and treated as non-existent. Those causes, so far as they exist, must establish their footing by direct investigation and research; carried on in the first instance apart from the long-recognized branches of science, until the time when they too have become sufficiently definite to be entitled to be called scientific. Outlandish territories may in time be incorporated as states, but they must make their claim good and become civilized first.

It is well for people to understand this definite limitation of scope quite clearly, else they wrest the splendid work of biologists to their own confusion—helped, it is true, by a few of the more robust or less responsible theorizers, among those who should be better informed and more carefully critical in their philosophizing utterances.

But, as is well known, there are more than a few biologists who, when taking a broad survey of their subject, clearly perceive and teach that before all the actions of live things are fully explained, some hitherto excluded causes must be postulated. Ever since the time of J. R. Mayer it has been becoming more and more certain that, as regards performance of work, a living thing obeys the laws of physics, like everything else; but undoubtedly it initiates processes and produces results that without it could not have occurred—from a bird's nest to a honeycomb, from a deal box to a warship. The behavior of a ship firing shot and shell is explicable in terms of energy, but the discrimination which it exercises between friend and foe is not so explicable. There is plenty of physics and chemistry and mechanics about every vital action, but for a complete understanding of it something beyond physics and chemistry is needed.

And life introduces an incalculable element. The vagaries of a fire or a cyclone could all be predicted by Laplace's calculator, given the initial positions, velocities and the law of acceleration of the molecules; but no mathematician could calculate the orbit of a common house-fly. A physicist into whose galvanometer a spider had crept would be liable to get phenomena of a kind quite inexplicable, until he discovered the supernatural, *i. e.*, literally superphysical, cause. I will risk the assertion that life introduces something incalculable and purposeful amid the laws of physics; it thus distinctly supplements those laws, though it leaves them otherwise precisely as they were and obeys them all.

We see only its effect, we do not see life itself. Conversion of inorganic into organic is effected always by living organisms. The conversion under those conditions certainly occurs, and the process may be studied. Life appears necessary to the conversion, which clearly takes place under the guidance of life, though in itself it is a physical and chemical process. Many laboratory conversions take place under the guidance of life, and, but for the experimenter, would not have occurred.

Again, putrefaction, and fermentation, and purification of rivers, and disease, are not purely and solely chemical processes. Chemical processes they are, but they are initiated and conducted by living organisms. Just when medicine is becoming biological, and when the hope of making the tropical belt of the earth healthily habitable by energetic races is attracting the attention of people of power, philosophizing biologists should not attempt to give their science away to chemistry and physics. Sections D and H and I and K are not really subservient to A and B. Biology is an independent science, and it is served, not dominated, by chemistry and physics.

Scientific men are hostile to superstition, and rightly so, for a great many popular superstitions are both annoying and contemptible; yet occasionally the term may be wrongly applied to practises of which the theory is unknown. To a superficial observer some of the practises of biologists themselves must appear grossly superstitious. To combat malaria Sir Ronald Ross does not indeed erect an altar; no, he oils a pond—making libation to its presiding genii. What can be more ludicrous than

the curious and evidently savage ritual, insisted on by the United States officers, at that hygienically splendid achievement, the Panama Canal—the ritual of punching a hole in every discarded tin, with the object of keeping off disease! What more absurd, again—in superficial appearance—than the practise of burning or poisoning a soil to make it extra fertile!

Biologists in their proper field are splendid, and their work arouses keen interest and enthusiasm in all whom they guide into their domain. Most of them do their work by intense concentration, by narrowing down their scope, not by taking a wide survey or a comprehensive grasp. Suggestions of broader views and outlying fields of knowledge seem foreign to the intense worker, and he resents them. For his own purpose he wishes to ignore them, and practically he may be quite right. The folly of negation is not his, but belongs to those who misinterpret or misapply his utterances, and take him as a guide in a region where, for the time at least, he is a stranger. Not by such aid is the universe in its broader aspects to be apprehended. If people in general were better acquainted with science they would not make these mistakes. They would realize both the learning and the limitations, make use of the one and allow for the other, and not take the recipe of a practical worker for a formula wherewith to interpret the universe.

What appears to be quite certain is that there can be no terrestrial manifestation of life without matter. Hence naturally they say, or they approve such sayings as, "I discern in matter the promise and potency of all forms of life." Of all terrestrial manifestations of life, certainly. How else could it manifest itself save through matter? "I detect nothing in the organism but the laws of chemistry and physics," it is said. Very well; naturally enough. That is what they are after; they are studying the physical and chemical aspects or manifestations of life. But life itself—life and mind and consciousness—they are not studying, and they exclude them from their purview. Matter is what appeals to our senses here and now; materialism is appropriate to the material world; not as a philosophy, but as a working creed, as a proximate and immediate formula for guiding research. Everything beyond that belongs to another region, and must be reached by other methods. To explain the psychical in terms of physics and chemistry is simply impossible; hence there is a tendency to deny its existence, save as an epiphenomenon. But all such philosophizing is unjustified, and is really bad metaphysics.

So if ever in their enthusiasm scientific workers go too far and say that the things they exclude from study have no existence in the universe, we must appeal against them to direct experience. We ourselves are alive, we possess life and mind and consciousness, we have first-hand experience of these things quite apart from laboratory experiments. They belong to the common knowledge of the race. Births, deaths and marriages are not affairs of the biologist, but of humanity; they went on before a single one of them was understood, before a vestige of science existed. We ourselves are the laboratory in which men of science, psychologists and others, make experiments. They can formulate our processes of digestion, and the material concomitants of willing, of sensation, of thinking; but the hidden guiding entities they do not touch.

So also if any philosopher tells you that you do not exist, or that the external world does not exist, or that you are an automaton without free will, that all your

actions are determined by outside causes and that you are not responsible—or that a body can not move out of its place, or that Achilles can not catch a tortoise—then in all those cases appeal must be made to twelve average men, unsophisticated by special studies. There is always a danger of error in interpreting experience, or in drawing inferences from it; but in a matter of bare fact, based on our own first-hand experience, we are able to give a verdict. We may be mistaken as to the nature of what we see. Stars may look to us like bright specks in a dome, but the fact that we see them admits of no doubt. So also consciousness and will are realities of which we are directly aware, just as directly as we are of motion and force, just as clearly as we apprehend the philosophizing utterances of an agnostic. The process of seeing, the plain man does not understand; he does not recognize that it is a method of ethereal telegraphy; he knows nothing of the ether and its ripples, nor of the retina and its rods and cones, nor of nerve and brain processes; but he sees and he hears and he touches, and he wills and he thinks and is conscious. This is not an appeal to the mob as against the philosopher, it is appeal to the experience of untold ages as against the studies of a generation.

How consciousness became associated with matter, how life exerts guidance over chemical and physical forces, how mechanical motions are translated into sensations—all these things are puzzling and demand long study. But the fact that these things are so admits of no doubt; and difficulty of explanation is no argument against them. The blind man restored to sight had no opinion as to how he was healed, nor could he vouch for the moral character of the Healer, but he plainly knew that whereas he was blind now he saw. About that fact he was the best possible judge. So it is also with "this main miracle that thou art thou, with power on thine own act and on the world."

But although life and mind may be excluded from physiology, they are not excluded from science. Of course not. It is not reasonable to say that things necessarily elude investigation merely because we do not knock against them. Yet the mistake is sometimes made. The ether makes no appeal to sense, therefore some are beginning to say that it does not exist. Mind is occasionally put into the same predicament. Life is not detected in the laboratory, save in its physical and chemical manifestations; but we may have to admit that it guides processes, nevertheless. It may be called a catalytic agent.

To understand the action of life itself, the simplest plan is not to think of a microscopic organism, or any unfamiliar animal, but to make use of our own experience as living beings. Any positive instance serves to stem a comprehensive denial; and if the reality of mind and guidance and plan is denied because they make no appeal to sense, then think how the world would appear to an observer to whom the existence of men was unknown and undiscoverable, while yet all the laws and activities of nature went on as they do now.

Suppose, then, that *man* made no appeal to the senses of an observer of this planet. Suppose an outside observer could see all the events occurring in the world, save only that he could not see animals or men. He would describe what he saw much as we have to describe the activities initiated by life.

If he looked at the Firth of Forth, for instance, he would see piers arising in the water, beginning to sprout, reaching across in strange manner till they actually join or are joined by pieces attracted up from

below to complete the circuit (a solid circuit round the current). He would see a sort of bridge or filament thus constructed, from one shore to the other, and across this bridge insect-like things crawling and returning for no very obvious reason.

Or let him look at the Nile, and recognize the meritorious character of that river in promoting the growth of vegetation in the desert. Then let him see a kind of untoward crystallization growing across and beginning to dam the beneficent stream. Blocks fly to their places by some kind of polar forces; "we can not doubt" that it is by helio- or other tropism. There is no need to go outside the laws of mechanics and physics, there is no difficulty about supply of energy—none whatever—materials in tin cans are consumed which amply account for all the energy; and all the laws of physics are obeyed. The absence of any design, too, is manifest; for the effect of the structure is to flood an area up-stream which might have been useful, and to submerge a structure of some beauty; while down-stream its effect is likely to be worse, for it would block the course of the river and waste it on the desert, were it not that fortunately some leaks develop and a sufficient supply still goes down—goes down, in fact, more equably than before: so that the ultimate result is beneficial to vegetation, and simulates intention.

If told concerning either of these structures that an engineer, a designer in London, called Benjamin Baker, had anything to do with it, the idea would be preposterous. One conclusive argument is final against such a superstitious hypothesis—he is not there, and a thing plainly can not act where it is not. But although we, with our greater advantages, perceive that the right solution for such an observer would be the recognition of some unknown agency or agent, it must be admitted that an explanation in terms of a vague entity called vital force would be useless, and might be so worded as to be misleading; whereas a statement in terms of mechanics and physics could be clear and definite and true as far as it went, though it must necessarily be incomplete.

And note that what we observe, in such understood cases, is an *interaction* of mind and matter; not parallelism nor epiphenomenalism nor anything strained or difficult, but a straightforward utilization of the properties of matter and energy for purposes conceived in the mind, and executed by muscles guided by acts of will.

But, it will be said, this is unfair, for we *know* that there is design in the Forth Bridge or the Nile Dam, we have seen the plans and understand the agencies at work; we know that it was conceived and guided by life and mind; it is unfair to quote this as though it could simulate an automatic process.

Not at all, say the extreme school of biologists whom I am criticizing, or ought to say if they were consistent, there is nothing but chemistry and physics at work anywhere; and the mental activity apparently demonstrated by those structures is only an illusion, an epiphenomenon; the laws of chemistry and physics are supreme, and they are sufficient to account for everything!

Well, they account for things up to a point; they account in part for the color of a sunset, for the majesty of a mountain peak, for the glory of animate existence. But do they account for everything completely? Do they account for our own feeling of joy and exaltation, for our sense of beauty, for the manifest beauty existing throughout nature? Do not these things suggest something higher and nobler and more joyous, something for the sake of

which all the struggle for existence goes on?

Surely there must be a deeper meaning involved in natural objects. Orthodox explanations are only partial, though true as far as they go. When we examine each particolored pinnule in a peacock's tail, or hair in a zebra's hide, and realize that the varying shades on each are so placed as to contribute to the general design and pattern, it becomes exceedingly difficult to explain how this organized cooperation of parts, this harmonious distribution of pigment cells, has come about on merely mechanical principles. It would be as easy to explain the sprouting of the cantilevers of the Forth Bridge from its piers, or the flocking of the stones of the Nile Dam by chemiotaxis. Flowers attract insects for fertilization; and fruit tempts animals to eat it in order to carry seeds. But these explanations can not be final. We have still to explain the insects. So much beauty can not be necessary merely to attract their attention. We have further to explain this competitive striving towards life. Why do things struggle to exist? Surely the effort must have some significance, the development some aim. We thus reach the problem of existence itself, and the meaning of evolution.

The mechanism whereby existence entrenches itself is manifest, or at least has been to a large extent discovered. Natural selection is a *vera causa,* so far as it goes; but if so much beauty is necessary for insects, what about the beauty of a landscape or of clouds? What utilitarian object do those subserve? Beauty in general is not taken into account by science. Very well, that may be all right, but it exists, nevertheless. It is not my function to discuss it. No; but it is my function to remind you and myself that our studies do not exhaust the universe, and that if we dogmatize in a negative direction, and say that we can reduce everything to physics and chemistry, we gibbet ourselves as ludicrously narrow pedants, and are falling far short of the richness and fullness of our human birthright. How far preferable is the reverent attitude of the eastern poet:

> The world with eyes bent upon thy feet stands in awe with all its silent stars.

Superficially and physically we are very limited. Our sense organs are adapted to the observation of matter; and nothing else directly appeals to us. Our nerve-muscle system is adapted to the production of motion in matter, in desired ways; and nothing else in the material world can we accomplish. Our brain and nerve systems connect us with the rest of the physical world. Our senses give us information about the movements and arrangements of matter. Our muscles enable us to produce changes in those distributions. That is our equipment for human life; and human history is a record of what we have done with these parsimonious privileges.

Our brain, which by some means yet to be discovered connects us with the rest of the material world, has been thought partially to disconnect us from the mental and spiritual realm, to which we really belong, but from which for a time and for practical purposes we are isolated. Our common or social association with matter gives us certain opportunities and facilities, combined with obstacles and difficulties which are themselves opportunities for struggle and effort.

Through matter we become aware of each other, and can communicate with those of our fellows who have ideas sufficiently like our own for them to be stimulated into activity by a merely physical process set in action by ourselves. By a timed succession of vibratory movements (as in speech and music), or by a static

distribution of materials (as in writing, painting and sculpture), we can carry on intelligent intercourse with our fellows; and we get so used to these ingenious and roundabout methods, that we are apt to think of them and their like as not only the natural, but as the only possible modes of communication, and that anything more direct would disarrange the whole fabric of science.

It is clearly true that our bodies constitute the normal means of manifesting ourselves to each other while on the planet; and that if the physiological mechanism whereby we accomplish material acts is injured, the conveyance of our meaning and the display of our personality inevitably and correspondingly suffer.

So conspicuously is this the case that it has been possible to suppose that the communicating mechanism, formed and worked by us, is the whole of our existence: and that we are essentially nothing but the machinery by which we are known. We find the machinery utilizing nothing but well-known forms of energy, and subject to all the laws of chemistry and physics—it would be strange if it were not so—and from that fact we try to draw valid deductions as to our nature, and as to the impossibility of our existing apart from and independent of these temporary modes of material activity and manifestation. We so uniformly employ them, in our present circumstances, that we should be on our guard against deception due to this very uniformity. Material bodies are all that we have any control over, are all that we are experimentally aware of; anything that we can do with these is open to us; any conclusions we can draw about them may be legitimate and true. But to step outside their province and to deny the existence of any other region because we have no sense organ for its appreciation, or because (like the ether) it is too uniformly omnipresent for our ken, is to wrest our advantages and privileges from their proper use and apply them to our own misdirection.

But if we have learned from science that evolution is real, we have learned a great deal. I must not venture to philosophize, but certainly from the point of view of science evolution is a great reality. Surely evolution is not an illusion; surely the universe progresses in time. Time and space and matter are abstractions, but are none the less real; they are data given by experience; and time is the keystone of evolution.

Thy centuries follow each other, perfecting a small wild flower.

We abstract from living moving reality a certain static aspect, and we call it matter; we abstract the element of progressiveness, and we call it time. When these two abstractions combine, cooperate, interact, we get reality again. It is like Poynting's theorem.

The only way to refute or confuse the theory of evolution is to introduce the subjectivity of time. That theory involves the reality of time, and it is in this sense that Professor Bergson uses the great phrase, "creative evolution."

I see the whole of material existence as a steady passage from past to future, only the single instant which we call the present being actual. The past is not non-existent, however; it is stored in our memories, there is a record of it in matter, and the present is based upon it; the future is the outcome of the present, and is the product of evolution.

Existence is like the output from a loom. The pattern, the design for the weaving, is in some sort "there" already; but whereas our looms are mere machines, once the guiding cards have been fed into them, the loom of time is complicated by a multitude

of free agents who can modify the web, making the product more beautiful or more ugly according as they are in harmony or disharmony with the general scheme. I venture to maintain that manifest imperfections are thus accounted for, and that *freedom* could be given on no other terms, nor at any less cost.

The ability thus to work for weal or woe is no illusion, it is a reality, a responsible power which conscious agents possess; wherefore the resulting fabric is not something preordained and inexorable, though by wide knowledge of character it may be inferred. Nothing is inexorable except the uniform progress of time; the cloth must be woven, but the pattern is not wholly fixed and mechanically calculable.

Where inorganic matter alone is concerned, there everything is determined. Wherever full consciousness has entered, new powers arise, and the faculties and desires of the conscious parts of the scheme have an effect upon the whole. It is not guided from outside, but from within; and the guiding power is immanent at every instant. Of this guiding power we are a small but not wholly insignificant portion.

That evolutionary progress is real is a doctrine of profound significance, and our efforts at social betterment are justified because we are a part of the scheme, a part that has become conscious, a part that realizes, dimly at any rate, what it is doing and what it is aiming at. Planning and aiming are therefore not absent from the whole, for we are a part of the whole, and are conscious of them in ourselves.

Either we are immortal beings or we are not. We may not know our destiny, but we must have a destiny of some sort. Those who make denials are just as likely to be wrong as those who make assertions: in fact, denials are assertions thrown into negative form. Scientific men are looked up to as authorities, and should be careful not to mislead. Science may not be able to reveal human destiny, but it certainly should not obscure it. Things are as they are, whether we find them out or not; and if we make rash and false statements, posterity will detect us—if posterity ever troubles its head about us. I am one of those who think that the methods of science are not so limited in their scope as has been thought: that they can be applied much more widely, and that the psychic region can be studied and brought under law too. Allow us anyhow to make the attempt. Give us a fair field. Let those who prefer the materialistic hypothesis by all means develop their thesis as far as they can; but let us try what we can do in the psychical region, and see which wins. Our methods are really the same as theirs—the subject-matter differs. Neither should abuse the other for making the attempt.

Whether such things as intuition and revelation ever occur is an open question. There are some who have reason to say that they do. They are, at any rate, not to be denied off-hand. In fact, it is always extremely difficult to deny *anything* of a general character, since evidence in its favor may be only hidden and not forthcoming, especially not forthcoming at any particular age of the world's history, or at any particular stage of individual mental development. Mysticism must have its place, though its relation to science has so far not been found. They have appeared disparate and disconnected, but there need be no hostility between them. Every kind of reality must be ascertained and dealt with by proper methods. If the voices of Socrates and of Joan of Arc represent real psychical experiences, they must belong to the intelligible universe.

Although I am speaking *ex cathedra*, as one of the representatives of orthodox sci-

ence, I will not shrink from a personal note summarizing the result on my own mind of thirty years' experience of psychical research, begun without predilection —indeed with the usual hostile prejudice. This is not the place to enter into details or to discuss facts scorned by orthodox science, but I can not help remembering that an utterance from this chair is no ephemeral production, for it remains to be criticized by generations yet unborn, whose knowledge must inevitably be fuller and wider than our own. Your president therefore should not be completely bound by the shackles of present-day orthodoxy, nor limited to beliefs fashionable at the time. In justice to myself and my coworkers I must risk annoying my present hearers, not only by leaving on record our conviction that occurrences now regarded as occult can be examined and reduced to order by the methods of science carefully and persistently applied, but by going further and saying, with the utmost brevity, that already the facts so examined have convinced me that memory and affection are not limited to that association with matter by which alone they can manifest themselves here and now, and that personality persists beyond bodily death. The evidence to my mind goes to prove that discarnate intelligence, under certain conditions, may interact with us on the material side, thus indirectly coming within our scientific ken; and that gradually we may hope to attain some understanding of the nature of a larger, perhaps ethereal, existence, and of the conditions regulating intercourse across the chasm. A body of responsible investigators has even now landed on the treacherous but promising shores of a new continent.

Yes, and there is more to say than that. The methods of science are not the only way, though they are our way, of arriving at truth.

Uno itinere non potest perveniri ad tam grande secretum.

Many scientific men still feel in pugnacious mood towards theology, because of the exaggerated dogmatism which our predecessors encountered and overcame in the past. They had to struggle for freedom to find truth in their own way; but the struggle was a miserable necessity, and has left some evil effects. And one of them is this lack of sympathy, this occasional hostility, to other more spiritual forms of truth. We can not really and seriously suppose that truth began to arrive on this planet a few centuries ago. The pre-scientific insight of genius—of poets and prophets and saints—was of supreme value, and the access of those inspired seers to the heart of the universe was profound. But the camp-followers, the scribes and pharisees, by whatever name they may be called, had no such insight, only a vicious or a foolish obstinacy; and the prophets of a new era were stoned.

Now at last we of the new era have been victorious; we inherit the fruits of the age-long conflict, and the stones are in our hands. Let us not fall into the old mistake of thinking that ours is the only way of exploring the multifarious depths of the universe, and that all others are worthless and mistaken. The universe is a larger thing than we have any conception of, and no one method of search will exhaust its treasures.

Men and brethren, we are trustees of the truth of the physical universe as scientifically explored: let us be faithful to our trust.

Genuine religion has its roots deep down in the heart of humanity and in the reality of things. It is not surprising that by our methods we fail to grasp it: the actions

of the Deity make no appeal to any special sense, only a universal appeal; and our methods are, as we know, incompetent to detect complete uniformity. There is a principle of relativity here, and unless we encounter flaw or jar or change, nothing in us responds; we are deaf and blind, therefore, to the immanent grandeur around us, unless we have insight enough to appreciate the whole, and to recognize in the woven fabric of existence, flowing steadily from the loom in an infinite progress towards perfection, the ever-growing garment of a transcendent God.

SUMMARY OF THE ARGUMENT

A marked feature of the present scientific era is the discovery of, and interest in, various kinds of atomism; so that continuity seems in danger of being lost sight of.

Another tendency is toward comprehensive negative generalizations from a limited point of view.

Another is to take refuge in rather vague forms of statement, and to shrink from closer examination of the puzzling and the obscure.

Another is to deny the existence of anything which makes no appeal to organs of sense, and no ready response to laboratory experiment.

Against these tendencies the author contends. He urges a belief in ultimate continuity as essential to science; he regards scientific concentration as an inadequate basis for philosophic generalization; he believes that obscure phenomena may be expressed simply if properly faced; and he points out that the non-appearance of anything perfectly uniform and omnipresent is only what should be expected, and is no argument against its real substantial existence.

Oliver Lodge

THE TEACHING OF COLLEGE BIOLOGY

In schools below college grade it is considered eminently desirable and necessary that the teacher shall have given some attention to the art of teaching. It is furthermore expected that he keep himself informed through meetings, reports, journals and discussions of progress in the art as well as the science he is expected to teach. He is expected to keep in touch with new ideas, in the subject matter and in the best methods of presenting them to his classes.

There appears to be a sharp distinction in this respect between these schools and colleges or universities. As a rule, college teachers are not expected to annoy themselves with principles of education or with methods of teaching. To do so is to ally oneself with prep. school ideas and associations. To be in open sympathy with any effort to arouse interest in the teaching side of one's profession is to lose caste with one's colleagues. Though primarily employed to teach, the consideration of one's specialty from the teaching standpoint is considered a necessary evil to be tolerated but not encouraged. Each new appointee is expected to adopt the university methods of his teacher or to stumble upon a plan which so frequently is a compromise between the limitations set by the institution and the bias of his training and experience, with little or no regard for the real needs of the student.

Very slowly there has developed a growing consciousness that the plans and methods that served so admirably during the last generation no longer met the needs of the college man or woman of the present day, particularly in the natural sciences. And the opinion has frequently been expressed that an exchange of ideas and experiences by men from different colleges or universities of the country would tend to clear the ground for an understanding of the nature and scope of the biology courses in schools of college grade. It was felt that the first effort should be directed toward a study of the introductory course in biology, the only one that the great majority of college students ever take.

During the summer of 1911 a number of biologists[1] met at Woods Hole, Mass., to discuss the nature and scope of the first year's or introductory course in the natural sciences in schools of college grade. It was agreed that very profound changes in the preparation of the student, in educational policies, in the attitude of the student and the public toward the science, and the great progress in the science itself, made it imperative that the college course be correspondingly modified in the light of these changes. It was also agreed that narrow standardization or uniformity was impossible.

The courses as outlined by each person present made it very evident that there was considerable agreement in certain fundamental principles and tendencies, namely: (1) a tendency away from the narrow study of comparative morphology; (2) a tendency to include fewer types, studied from a wider viewpoint; (3) a tendency to emphasize the study of living organisms in relation to their environment; (4) a tendency to emphasize physiological processes; (5) to include the consideration of the relation of living organisms to man; (6) to include the consideration of general and fundamental phenomena, and some of the big problems that biologists are endeavoring to solve.

Unfortunately time did not permit an adequate discussion of what appears to the writer to be a very important phase of the problem, namely, to what extent should the student be made to realize the methods used in the investigation of biologic phenomena, and the nature of the value of biologic evidence. It would be extremely useful if Professors Conklin, Calkins, Lefevre, McClung and others present at the meeting could be persuaded to make their plans and experiences public.

[1] The men present were: Professors Calkins of Columbia, Conklin of Princeton, Goldfarb of the College of the City of New York, Kellicott of Goucher College, Knower of Cincinnati, Lefevre of Missouri, Lewis of Wisconsin, McClung of Pennsylvania, Montgomery (who recently died), Moore of Washington University (St. Louis), Parker of Harvard, Patterson of Texas, Pike of Columbia.

The results of the meeting suggested the desirability of obtaining certain data from a larger number of institutions, in the belief that they might serve as a basis for a more general and open discussion of a difficult and important problem. Fully conscious of the limitations of such tabulated data, they are nevertheless submitted, as obtained from over fifty colleges and universities. Over ten, not included in the tables to follow, were so incomplete as to make their inclusion of very questionable value. By request, the names of the contributors are not mentioned. I wish to express my thanks to all who so kindly cooperated in furnishing the data called for.

OPTIONAL OR REQUIRED INTRODUCTORY BIOLOGY

The term biology is here used in a very loose sense to mean any introductory course in the natural sciences offered in colleges.

Number of Colleges	
40	Biology is required at least of certain groups of students.
4	Biology is not required, optional only.
1	Biology is not offered at all, except one term of elementary physiology.

Number of Colleges	Length of Course	
5	one half year	Required
33	one year	Required
4	one year	Optional
1	two years	Required
1	three years	Required

PROPORTION OF THE STUDENT BODY WHO TAKE INTRODUCTORY BIOLOGY

A very small part of the student body take or have taken this introductory course, as shown by the following table:

No. of Colleges	Per Cent. Students
2	100
1	90
3	50
9	38
2	20
14	10
2	2½
7	*

* Doubtful.

STUDENTS IN INTRODUCTORY COURSE GROUPED ACCORDING TO THEIR OFFICIAL CLASSES

Per cent. of class who take the biology	100	90	80	70	60	50	40	30	20	10	0
	Number of Colleges										
Freshmen	0	3	2	3	5	7	3	1	3	2	10
Sophomores	0	0	1	1	3	4	2	4	11	7	6
Juniors	0	0	1	1	2	1	2	1	5	15	11
Seniors	0	0	0	0	0	0	1	3	2	12	21
Freshmen and Sophomores	4	6	10	6	5	0	2	0	2	1	3
Juniors and Seniors	2	1	1	1	0	1	3	8	10	6	6

It is perfectly clear that so far as these colleges are concerned the great majority, *i. e.*, 30 to 100 per cent., of the classes in introductory biology are freshmen and sophomores, and that 0 to 30 per cent. are from the junior and senior classes. The presumption, of course, is that the course is adapted to the needs of the lower class men and not to advanced or university students.

DATA CONCERNING THE SUBJECT MATTER OF THE COURSE

1. The nature of the introductory course in the different colleges is given in the following tables.

Invertebrate zoology in 3 colleges.
Vertebrate zoology in 0 college.
Zoology (vertebrate and invertebrate) in 23 colleges.
Animal and plant (biology) in 16 colleges.
Botany in 2 colleges.

In a few colleges the student is permitted to choose between a year's course either in zoology or in botany.

2. The character of the course is to some extent indicated by the kind and number of "types" used. The returns show that the *one type course*, somewhat like Huxley's crayfish, is not used in any college; the *few type course*, like the Sedgwick and Wilson biology, is used in 11 colleges; the *many type course*, like the Parker and Haswell zoology, is used in 28 colleges. A distinct modification of the last kind of course consists in greatly emphasizing one, usually a vertebrate organism, and studying other types in less detail, and fewer in number. Seven colleges adopted this kind of course.

The following table gives an idea of the number of types used in different colleges.

ZOOLOGY COURSE

Number of Types	Number of Colleges
15	2
14	1
13	0
12	5
11	5
10	9
9	5
8	5
7	2
6	4
5	1
	Total 23 colleges

PLANT AND ANIMAL COURSE

Number of Animal Types	Number of Plant Types	Number of Colleges	Total Number of Types
10	4	1	14
8	4	1	
9	3	1	12
7	5	1	
7	4	2	11
7	3	1	
6	4	1	10
5	5	3	
8	1	1	
5	4	1	9
3	6	1	
3	4	1	7
2	4	1	6
		Total 16 colleges	

These tables clearly show the preponderance of the zoologist and of zoological types, even in so-called biology courses. They also indicate the significant departure from the study of a representative type from each phylum, in the direction of limiting the number except in courses mistakenly designed to prepare students for medicine.

In the following table the zoologic types are grouped according to their frequency:

	Colleges
Protozoan type used in	38
Cœlenterate	37
Annelid	36
Crustacea	33
Amphibia	31
Insecta	25
Mollusc	22
Echinoderm	19
Flat worm	16
Fish	13
Sponge	12
Mammal	11
Embryology	8
Round worm	4
Bird	3
Reptile	3
Man	1

This table also shows that there is a distinct tendency not to include in the course a type from each phylum. It is far more significant as indicating the choice of types that are believed to have the greatest teaching value, as judged by teachers in different colleges. The first half of the table includes the types that will probably be chosen more and more for the kind of course under discussion.

When the botanical types are grouped according to their frequency in the sixteen colleges, it is found that the

Fern is used in 13 colleges,
Yeast in 11 colleges,
Algæ in 11 colleges,
Flowering plants in 9 colleges,
Fungi in 8 colleges,
One-celled plants other than the above in 3 colleges,
Fern and lower plants only in 5 colleges.

This table shows that plant phenomena are taught in most of the colleges from representatives of all the main plant groups, namely, bacteria, algæ, fungi, ferns and flowering plants, that economically important plants are given splendid recognition. This distribution of types stands in marked contrast to the zoologic courses in which invertebrate types predominate.

It may be interesting to note that only seventeen colleges used the well-known ascending or evolutionary order in the study of the types, three colleges used the descending or so-called pedagogic order, *i. e.*, from organisms best known to the student to those least known, or those whose study involves the greatest technical difficulties. In fourteen colleges an introductory type is studied intensively to acquaint the student with biologic apparatus and methods, and to afford a basis for comparison with subsequent types. The ascending order in most colleges follows this introductory type. In four colleges only the type method of instruction is not used at all, as splendidly illustrated in Needham's book.

TIME IN HOURS DEVOTED TO THE COURSE

There is an extremely wide range in time and in the distribution of the time to lecture, recitation, laboratory and field work. The following tables give the detailed information.

FOR ONE YEAR

Hours per Week	Number of Colleges
11	1
10	3
9	1
8	1
7	4
6	**10**
5	**7**
4	**6**

FOR ONE HALF YEAR

8	1
5	1
4	1
3	2

Far more significant than the mere fact that most colleges provide four to six hours per week for one year, which arrangement seems to be the one more and more in vogue, are the facts shown in the next table, which gives the time devoted to lecture, recitation and laboratory.

Hours per week	0	*	½	1	2	3	4	5	6	7	8	9
To lecture	0	2	**3**	**13**	**19**	4	0	0	0	0	0	0
To recitations	**9**	**7**	**4**	**18**	2	0	1	0	0	0	0	0
To laboratory	0	0	0	0	**13**	**10**	**8**	2	4	0	3	1

* Occasional.

It will be observed that in several colleges as much time is given to lecturing about things as to the study of the things them-

selves. In eighteen colleges two hours a week are spent in lectures and two hours a week in the laboratory; in three colleges three hours are devoted to lectures and the same time to laboratory. This is a regrettable survival of the so-called German university system.

In four colleges no recitation or quiz is given at all. In thirteen only an occasional recitation is held; in three colleges not more than a half hour, either each week or at various intervals, but not extending beyond this time.

The following table supplements the above and brings out in sharp relief the overemphasis of the lecture and the very inadequate attention to the recitation.

Number of Hours in			Number of
Lecture	Recitation	Laboratory	Colleges
1	1	2	2
½	½	2	2
1	1	3	2
1	1	4	5
1	1	5	1
1	1	6	2
1	2	8	1
2	0	2½	5
2	*	3	4
2	1	2	2
2	0	4	1
2	1	4	2
2–3	0–1	5–9	1
2	0	6	1
2	0	8	1
2	1	8	1
3	0	3	1
3	½	3	2
3	1	6	1
3		2	2
½	2	5	1
few	4	3	1

* Occasional.

The following table gives an idea of the frequency that certain topics are considered in the course, either in the laboratory lecture, essays or assigned readings.

32 colleges, the theory of evolution.
31 colleges, heredity.
29 colleges, comparative anatomy of invertebrates.
24 colleges, comparative anatomy of vertebrates.
22 colleges, histology.
21 colleges, bacteriology and sanitation.
19 colleges, botany.
15 colleges, experimental zoology.
15 colleges, experimental embryology.
13 colleges, paleontology.

There are twenty-five colleges that treat of the economic or applied biology, eighteen of which treat this phase of the course in lectures only, four in lectures and laboratory, three in lectures, laboratory and practical or field work. Four colleges do not include economic aspects of the science in the course.

I had hoped to obtain information with reference to the manner and the extent to which this aspect of the problem was considered. But the returns did not lend themselves to tabulation.

ARTICULATION WITH SECONDARY SCHOOL BIOLOGY

In the College of the City of New York the students in the introductory course include those who have not had a high-school course in biology and those who have had such a course. It has been our experience that the one group is not appreciably better informed or better equipped to attack the subject, nor do they appear to do any better than the other group of students. It is not my purpose to make any reflection upon the excellent work done by exceedingly able and conscientous teachers in the high schools. I merely wish to state that, so far as our experience goes, it is altogether probable that the college course may safely ignore any training or equipment based upon the high-school course in biology. Furthermore, since every tendency indicates a continued independence of the high-school courses from the domination or educational policies of colleges and universities, it seems safe to conclude that any articulation with the high schools is inadvisable.

BIOLOGY TEACHING IN COLLEGES

It is now generally agreed that every college man or woman should have had at least one year's college biology. This plan is now adopted in nearly all colleges. It is also agreed that in order to reach the larger body of students and to make possible later special-

ization that the introductory course should be offered as early in the college curriculum as possible.

Since an exceedingly small proportion of the students continue the study of biology, namely, those preparing for medicine or teaching, and since the great majority leave college without any further training or acquaintance with the subject, the opinion seems to prevail that the introductory course should be a rounded one, that it should give a first-hand acquaintance with *living organisms*, in relation to their environment, an adequate idea of the larger and fundamental problems of the biologist and, above all, an idea of the general methods used in biologic investigations.

While there is considerable range of opinion with respect to the time required for the course, there is an undoubted tendency to limit the course to five or six hours a week for one academic year.

Upon the broad lines just suggested there is a general agreement, beyond these there is a healthy divergence of opinion, particularly upon the nature and the content of the course. There has been an undoubted tendency away from the narrow study of comparative morphology, the standard course of a generation ago, toward an increasing emphasis upon an adequate understanding of fundamental biologic phenomena, as we understand the term to-day, of the unit of the organism, the cell, the organism, and the fundamental processes characteristic of living things in general. To give such a course it has been found increasingly expedient to study representatives of animal and plant kingdoms. There are very many eminent teachers who believe that, on account of practical difficulties, it were better to use animal organisms only and to develop the fundamental properties of living things from zoologic types only. But these teachers are in nearly every instance zoologists.

The chief kinds of courses show considerable variation. There are courses like the almost abandoned narrow comparative morphology, others in which attention is directed to the functioning of the mechanisms studied and others in which the emphasis is placed upon the laws which living things obey, and only sufficient attention given to the structures involved as will make the understanding of these laws possible. Professor Kofoid's course, as I understand it, is one such course. This idea carried to its extreme is illustrated in courses that follow more or less closely the Jordan and Kellogg evolution book. Where the endeavor is to offer an abbreviated course usually covering one semester and to give the student an idea of fundamental principles a course somewhat along the lines of Sedgwick and Wilson's biology is followed. Professor Needham's course in biology is too well known to need extended comment. It is another fine contribution and merits further trial.

There can be no question but that the trend of thought is in the direction of giving the student a rounded and definite view of the world of living things, that the student who pursues the subject no further may carry with him an adequate knowledge of the world of living beings, and that the student who intends to make a more intensive study of the biologic sciences may have a sufficient background for the choice of his electives as his interest or needs may demand.

With a changed viewpoint in the matter of the scope of the course has come an increasing appreciation of the value of the study of *living things*. They are no longer thought unworthy of serious study, to be left to teachers of kindergartens and elementary schools. It is no longer deemed necessary to depend exclusively upon foul-smelling, often distorted and discolored preserved specimens for an understanding of a living organism. At the last meeting of the representatives of the colleges of the Middle States and Maryland there was a wholesome and surprising agreement on the important place that living organisms should hold in our biologic courses.

With an appreciation of the desirability of studying living organisms the importance of local or well-known forms has become apparent. The choice of a type has unfortunately been too frequently determined by the

author of the laboratory guide book, rather than the needs of the student. Where there is a choice between two forms that are equally good in developing the ideas of structure or physiological processes, the local or more generally known form should always be preferred. Obvious as this may appear, there are a number of instances where exotic or marine forms are used where fresh-water local specimens are available.

The data submitted showed that there was a very wide range in the time given to the course, that there was nevertheless a tendency to limit the number of hours to five or six a week for one year. Whatever the number of hours may be, there is, in so many colleges, an undue importance placed upon the value of lectures as against the value of self-expression either in the laboratory or in the recitation. If our message is to study nature, not books, even if it appears necessary to study nature through the artificial medium of the laboratory, as much time should be given to the study of organisms at first hand as circumstances warrant. It is exceedingly difficult to state what proportion of the time should be spent in the lecture, laboratory and recitation. It is easier to state what is wrong than what is right. It seems to the writer at least that two hours in the lecture room and two hours in the laboratory placed a disproportionate emphasis upon a knowledge about, rather than of, nature. Yet in twelve colleges this is the situation.

Even more surprising is the lack of appreciation of the value of the recitation in such an introductory course. In nine colleges, for example, no opportunity is offered for self-expression on the part of the student, or for determining how far the student has grasped the ideas, or to what extent the course is adapted to the needs of the particular group of students, but more important even than these is the opportunity offered by the properly conducted recitation to let the student appreciate the method of scientific thinking and the numberless unanswered problems that the biologist is wrestling with. In seven colleges only occasional recitations are held; in four colleges the recitations extend not more than a half hour a week.

It is to be hoped that the reserve that has so long prompted many excellent teachers and biologists to withhold from their colleagues the results of their many years of experimentation and thought upon the teaching of introductory biology, may be set aside and that appropriate means be found for an exchange of experiences. If arousing and developing a wholesome interest in biology is an important part of our duties in the colleges or universities, should we not cooperate in aiding one another in this important work. At worst, we can agree to differ.

A. J. Goldfarb
College of the City of New York

MEXICAN ARCHEOLOGY AND ETHNOLOGY

A greater impetus will be given to the International School of American Archeology and Ethnology in the city of Mexico in this, the fourth year of its existence. The members have been added to and the fund for its use will be increased so as to permit of larger activities and explorations. The school was founded in 1910 by the governments of Mexico and Prussia, Columbia University, Harvard University, the University of Pennsylvania and the Hispano Society of America, under the initiative of Columbia. In the second year of the school the government of Russia, through the Imperial Academy of Sciences, and the government of Bavaria, joined the school, and in the third year the government of Austria and the city of Leipsic, through its ethnological museum, joined it. During the first year the budget of the school, including salaries and fellowships, amounted to $6,000, in the second and third years to $10,000 each, and in the coming year it will be $12,000, of which amount Mexico contributes $3,000 and two $500 fellowships. No elementary or popular instruction is given in the school, but opportunity is offered to advanced students to familiarize themselves with the problems of Mexican archeology and ethnology, and to understand researches in these fields. The objects collected by the school are

placed at the disposal of the National Museum of Mexico, to make such selections as it thinks desirable and the remainder becomes the property of the patrons of the school. The first director of the school was Professor Edward Seler, of Berlin, appointed by Prussia; the second was Professor Franz Boas, of New York, appointed by Columbia; the third was Professor Jorge Engerrand, of Mexico, appointed by Mexico, and the fourth will be Professor A. M. Tozzer, appointed by Harvard.

It has been the endeavor of the successive directors to organize the work of the school in such a way as to concentrate the energies of the school on a few carefully selected tasks. Professor Seler undertook an investigation of the ruins of Palenque and of some of the less-known ruins of Yucatan, and, after the completion of this work, inaugurated investigations on the archeological types of the valley of Mexico. In the same year Professor Boas devoted some time to linguistic studies on the dialects of the Nahua. In the second year the archeological studies in the valley of Mexico were continued, and a series of stratigraphical examinations of sites was undertaken. These led to the discovery of a regular sequence of three cultural types, the presence of which was known before, although their relative ages had not been determined, and pointed out the need of extended stratigraphical investigations in the valley of Mexico. Remains were found deep below the level of the lakes of the valley of Mexico, showing the great antiquity of the various types of culture. On the hills, sites were discovered in which the oldest type of culture appeared on the surface. The investigation of the dialects of Mexico was continued, particularly through studies on the southern dialects of the Nahua. Studies on Mexican folklore were also taken up, which yielded the most abundant and interesting results, suggesting the most curious interrelations between the folklore of Spain, Africa and America, and suggesting a much more important influence of Spanish folklore upon American tradition than has generally been assumed to exist. In the third year, Professor Engerrand continued similar lines of work. Under his direction the stratigraphical work was continued on a large scale in the valley of Mexico, and yielded most interesting results, clearing up still further the historical relation between the three cultural types. A comparative study was also made in the state of Colima. One of the fellows of the school who worked under his direction made a large folkloristic collection in Oaxaca, and studied the Huave, one of the isolated languages of that area, which he proved to be related to the Mixe. Another fellow continued his studies on the language, religion and folklore of the Tepecanos, a Pima tribe in northern Jalisco. The importance of the stratigraphical work conducted by the school has proved so great that the Geological Institute of Mexico is now continuing this enterprise on a large scale by means of borings. During the coming year, under the direction of Professor Tozzer, the stratigraphical work in the valley of Mexico will be continued, and the study of folklore will receive particular attention. The studies on the Nahua dialects will also be continued.

THE AMERICAN FISHERIES SOCIETY

THE forty-third annual meeting of the American Fisheries Society was held in Boston from September 8 to 11 under the presidency of Dr. C. H. Townsend, of the New York Aquarium. Dr. Henry B. Ward, of the University of Illinois, was vice-president, and the vice-presidents of divisions were as follows: Fish Culture, James Nevin, Madison, Wis.; Aquatic Biology and Physics, L. L. Dyche, Pratt, Kan.; Commercial Fishing, W. J. Hunsaker, Saginaw, Mich.; Angling, H. Wheeler Perce, Chicago, Ill.; Protection and Legislation, Dr. T. S. Palmer, Washington, D. C. The program of scientific papers was as follows:

William P. Seal: "Suggestions of possible Interest to the American Fisheries Society and to Fish Commissions."

Dr. C. H. Townsend, director, New York Aquarium: "The Private Fish Pond—a neglected resource." Recent Progress in Oceanography.

F. F. Dimick, secretary, Boston Fish Bureau: "The Fish Trade Organizations."

Dr. H. M. Smith, commissioner, U. S. Bureau of Fisheries: "The Need for a National Institution for the Technical Instruction of Fisherfolk."

L. L. Dyche, state fish and game warden, Kansas: "One Year's Work at the Kansas Fish Hatchery," "The Possibilities of an Acre Fish Pond."

Jacob Reighard: "A Plea for the Preservation of Records concerning Fish," "Improvement of Fishing through a Knowledge of the Breeding Habits of Fish."

Phil C. Zalsman: "Experiments in Fish Culture while in the Employment of the Michigan and Wisconsin Fish Commissions."

Charles H. Nerley: "Small Mouth Black Bass."

J. P. Snyder: "Notes on Striped Bass."

J. T. Nichols: "Concerning Young Bluefish."

Dr. George W. Field, chairman, Massachusetts Fish and Game Commission: "The Alewife Fishery of Massachusetts."

Dr. T. H. Bean, state fish culturist, New York: "The Rearing of Small-mouthed Black Bass."

N. R. Buller, commissioner, Pennsylvania Fisheries Department: "The Work of the Pennsylvania Fisheries Department."

Charles G. Atkins, superintendent, U. S. Fisheries Station, Craig Brook, Maine: "The Atlantic Salmon."

Dr. Irving H. Field, Clark University, Worcester, Massachusetts: "The Development of the Salt Water Mussel Industry."

Professor Henry B. Ward, Urbana, Illinois: "Fish Refuges."

W. E. Meehan, director, Philadelphia Aquarium: "The Establishment of an Aquarium in Philadelphia."

Professor E. E. Prince: "Some Animals and Conditions Inimical to Fish Eggs and Larvæ in the Sea," "A Perfect Fish Pass; Some Suggestions as to Defects in Fish Passes and How to Overcome Them."

Henry C. Rowe, president, Oyster Growers and Dealers Association of North America: "The Oyster Industry."

David L. Belding, biologist, Massachusetts Fish and Game Commission: "Conditions Influencing the Growth of Clams (*Myra arenaria*)."

Professor G. H. Parker, Harvard University: "The Senses of Fishes."

The next annual meeting will be held in New Orleans beginning on September 30, 1914.

CHEMISTRY AT THE ATLANTA MEETING OF THE AMERICAN ASSOCIATION

At the meeting of the American Association for the Advancement of Science to be held in Atlanta, it is planned to hold sessions of Section C (Chemistry), of which no sessions were held at Cleveland in 1912. The general idea is to endeavor to have before Section C papers on chemical topics of wide and general interest, especially to workers in other branches of science and to laymen, leaving to the American Chemical Society the field which they already occupy, namely, the presentation of chemical papers to and for chemists. In pursuance of this plan it is proposed to have some short addresses, each of which will either deal with some general topic or be of the nature of reports of recent progress in some of the large branches of the subject of chemistry. A second day may, if it prove desirable and practicable, be devoted to a joint meeting of Section C with the local sections of the American Chemical Society, in which case papers dealing with more special subjects would be read. The secretary of Section C is Dr. John Johnston, Geophysical Laboratory, Washington, D. C.

SCIENTIFIC NOTES AND NEWS

The University of Birmingham on September 11 conferred its doctorate of laws on the following foreign representatives in attendance at the meeting of the British Association: Madame Curie (Sorbonne, Paris), Professor H. A. Lorentz (Leyden), Professor Keibel (Freiburg), Professor R. W. Wood (Johns Hopkins) and Professor Svante Arrhenius (Stockholm).

The evening discourses at the Birmingham meeting of the British Association were given by Sir Henry Cunynghame, of the Home Office, on "Coal Dust Explosions and the Means of Preventing Them," and by Dr. Smith Woodward, F.R.S., of the British Museum, on "Missing Links among Extinct Animals."

Dr. Wilhelm Ostwald, the distinguished physical chemist and philosopher, celebrated his sixtieth birthday on September 2.

DR. G. S. FULLERTON, professor of philosophy at Columbia University and exchange professor with Austria, will lecture at Vienna six weeks in the autumn and six weeks in the spring. He will lecture also at Graz and Innsbruck.

THE Walker prizes in natural history of the Boston Society of Natural History have been awarded this year as follows: the first prize, amounting to $100, to Dr. Reynold A. Spaeth, for his essay on "An Experimental Study Concerning the Chromatophores of Fishes," and the second prize of $50 to Professor O. D. Von Engeln, for his essay on the "Effects of Continental Glaciation on Agriculture." Prizes for 1914 and 1915 will be awarded for original and unpublished research work in any biological or geological subject. The memoirs must be in the hands of the secretary on or before April 1.

THE University of Munich has awarded a prize of 3,000 Marks to Dr. Joseph Golling for his research entitled anthropological investigations on the bones of the nose in man.

PROFESSOR NEUBERG has been appointed a demonstrator in the chemistry division of the Kaiser Wilhelm Institute for Experimental Therapy in Dahlem, near Berlin.

DR. R. LÖWENHERZ, docent for chemistry at Königsberg, has been appointed curator of the chemical museum of the Berlin Technological Institute.

PROFESSOR A. KOLB, of the Technical Institute of Darmstadt, has retired to engage in industrial chemical work in Berlin.

MR. H. L. VIERECK, formerly with the Bureau of Entomology at Washington, is at present working with the Minnesota state entomologist, Professor F. L. Washburn.

THE annual meeting of the Association of Military Surgeons of the United States was held in Denver, Colo., September 16–19, under the presidency of Surgeon W. C. Braisted, U.S.N.

AT the twenty-third annual meeting of the American Electrotherapeutic Association, held in New York on September 2, 3 and 4, the following officers were elected: president, Dr. George E. Pfahler, of Philadelphia; vice-presidents, Dr. Albert C. Geyser, of New York, Dr. Frank B. Granger, of Boston, Dr. John D. Torbett, of Marlin, Texas, Dr. William L. Clark, of Philadelphia, and Dr. Frederick P. Tice, of Roanoke, Va.

PRESIDENT SCHURMAN, who has been on leave of absence from Cornell University for the past year, representing the United States at Athens as minister to Greece, has returned to the university.

PROFESSOR B. K. EMERSON, owing to an injury to his knee, will be unable to conduct the New England Intercollegiate Geological Excursion which was planned for the vicinity of Dalton, Mass., and consequently the meeting will not be held this year.

WE learn from *Nature* that a ship has been purchased for an Austrian expedition to the South Polar regions, and that funds are being collected in aid of the object. The expedition is to be under the leadership of Dr. F. König, of Graz, and the proposal is that it shall leave Trieste in May next. A large donation to the funds has been given by the Vienna Academy of Science, and the Austrian Geographical Society has promised an annual subsidy.

THE Philadelphia Alumni Society of the medical department of the University of Pennsylvania has issued an appeal for funds to endow a scholarship which it is planned to establish in memory of the late Dr. Roland G. Curtin.

KING GEORGE received recently at Buckingham Palace the members of the Scott Expedition and presented them with the antarctic medal and clasp. He also presented Lady Scott, Mrs. Wilson, Mrs. Evans and Mrs. Brissenden with the medal and clasps which had been awarded to their late husbands, and to Mrs. Bowers the medal and clasp awarded to her son, the late Lieut. H. R. Bowers. At the request of the late Captain Oates's mother, the medal and clasp awarded to her son were received on her behalf by Commander Evans.

A MEMORIAL to the lost Russian explorer, Baron E. von Toll, is to be set up on the west

coast of Kotelnyi Island, in the New Siberia group—the starting-point of the explorer and his companions on their last journey.

Professor John Milne has left his books, albums and scientific instruments relating to seismology to the British Association; and subject to his wife's interest £1,000 to the chairman of the seismology committee of the association, for the study of earth physics.

Dr. Alexander Macfarlane, formerly instructor in physics in Edinburgh University and professor of physics in the University of Texas, recently residing in Chatham, Ontario, known for his contributions to vector analysis and quaternions, died on August 28, aged sixty-two years.

Dr. Hugh Marshall, F.R.S., professor of chemistry in University College, Dundee, died on September 6, aged forty-five years.

Dr. George Friedrich Kinkelin, the geologist of the Frankfort Senckenberg Natural History Society, has died at the age of seventy-eight years.

Dr. Bernhard Bardenhauer, professor of surgery at Cologne, has died at the age of seventy-three years.

Dr. Wilhelm Muthmann, professor of chemistry in the Technical School at Munich, known for his work on the rare earths, has died at the age of fifty-two years.

Dr. Friedrich Seiler, professor of pharmaceutical chemistry at Lausanne, has died at the age of fifty years.

Dr. O. M. Reuter, emeritus professor of zoology in the University of Helsingfors, died on September 2, at the age of sixty-three years.

Columbia University opened its 160th academic year on September 24, when Professor James F. Kemp, head of the department of geology, made the address, his subject being "The Appeal of the Natural Sciences."

The sum of 90,000 francs has been bequeathed to the Pasteur Institute at Paris for the founding of a prize for the best original work in the treatment of meningitis.

The sixth annual meeting of the Association of Official Seed Analysts will be held in Washington, D. C., November 14 and 15, at the time of the meetings of the Agricultural Group of Societies.

The Prussian ministry of education, which a short time ago made grants of money to the university clinics at Berlin, Halle and Kiel, enabling them to procure radium or mesothorium for the treatment of cancer, is now said to have placed $200,000 in the estimates of next year for further purchases.

It is stated that the Maharaja Scindia of Gwalior is giving special attention to the archeological relics and treasures in his state, and is taking steps to create an archeological department in Gwalior. In furtherance of this object he has sought the advice and co-operation of the director-general of archeology in India.

Lord Murray, of Elibank, has concluded with the government of Ecuador a contract for the development of the oil resources of the republic. Under this contract Lord Murray undertakes to spend £100,000 within ten years in exploring for oil in Ecuador. Work is to begin within a year of the publication of the proposed law, and competent geologists and engineers are to be employed who are to supply the government with detailed maps of the country they survey and to keep the government specially informed as to discoveries of artesian water.

A further grant of £5,000, making £10,000 in all, has been made by the federal government of the commonwealth of Australia towards completing the work of the Mawson Antarctic Expedition and bringing the explorers back.

The *Journal* of the American Medical Association says that the initiative of the medical profession of Philadelphia and Pennsylvania has brought into legal existence large new institutions that will bring the city and state well into the advance along social lines: Mentally defective women of childbearing age, of whom at least 15,000 are known to be within the state, will now be permanently segregated in a great farm colony in a remote mountain forest reserve, thus preventing further multi-

plication. A home for alcohol and drug habitués, long agitated, is provided for in another forest reserve as, elsewhere, is an industrial home for women.

IT is stated in *Nature* that the Italian archeological mission to Crete, under the leadership of Professor Halbherr, announces the discovery at Cortina of a temple dedicated to Egyptian deities, bearing the dedication by Flavia Philyra, the foundress. In the inner cella were found images of Jupiter, Serapis, Isis and Mercury, with fragments of a colossal statue, supposed to be that of the foundress. A little flight of steps leads down to a subterranean chamber in which ceremonies of purification were performed. The excavation of the numerous prehistoric sites in the island of Malta is being actively prosecuted under the direction of Professor T. Zammit. The most important discovery is that of a series of well tombs of the Punic type at the Kallilia plateau, northwest of Rabat. A large number of skeletons, with pottery, lamps, spindle-whorls and a circular bronze mirror, has been unearthed. A partial exploration of the Ghar Dalam cave, conducted by Professor Tagliaferro and Mr. C. Rizzo, produced bones of a hippopotamus and a deer, above which lay a quantity of prehistoric sherds. The museum, by the bequest of the late Mr. Parnis, has received a large collection of books about Malta and numerous antique objects.

UNIVERSITY AND EDUCATIONAL NEWS

IT is announced that the scheme for the establishment of a school of tropical medicine in Calcutta is now so far advanced towards fulfilment that there is every reason to hope that it will be opened in the autumn of next year.

PROFESSOR ALEXANDER T. ORMOND has resigned the McCosh professorship of philosophy at Princeton University to accept the presidency of Grove City College.

PROFESSOR ALEXANDER SMITH, head of the department of chemistry in Columbia University, who has been elected professor of chemistry at Princeton University, will not assume his new duties until the academic year 1914–1915.

THE vacancy created at Vassar College by the resignation of Professor Clark Wells Chamberlain, in order to take the presidency of Denison University, has not been filled; Associate Professor Edna Carter will act as head of the department of physics for the present year.

AT Lehigh University the following promotions in the faculty are announced: George C. Beck, to be assistant professor of quantitative analysis; Sylvanus A. Becker, assistant professor of civil engineering; Joseph B. Reynolds, assistant professor of mathematics and astronomy; Rollin L. Charles, assistant professor of physics; Stanley J. Thomas, instructor in biology. The following appointments have been made: Ferdinand F. Hintze, assistant professor of geology; Siegfried Fischer, instructor in metallurgy; Wallace G. Matteson, instructor in geology; Edgar C. Weinsheimer, instructor in geology; M. S. Knebelman, instructor in mathematics; James B. Arthur, instructor in electrical engineering.

AT Rutgers College Stanley E. Brasefield, Ph.D. (Cornell), and William Beverly Stone, Ph.D. (Univ. of Va.), have been appointed assistant professors of mathematics.

L. C. PLANT, who has been at the head of the department of mathematics in the University of Montana for the past six years, has resigned, to accept the position of head of the department of mathematics in the Michigan Agricultural College. He has been succeeded by Dr. N. J. Lennes, of the department of mathematics of Columbia University.

DR. WALTER KRUSE, of Bonn, has been appointed professor of hygiene at Leipzig.

DISCUSSION AND CORRESPONDENCE

BIT OF HISTORY

IN the issue of SCIENCE for August 15, 1913, there is quoted from *The Independent* of fifty years ago the statement that "Professor Wolcott Gibbs" had been chosen to the Rumford

chair at Harvard College together with the well-worn comment of

> Columbia College a year or two since refused to appoint him to a chemical professorship. Because he did not understand chemistry? No; because he was a Unitarian!

At the time of the death of Professor Gibbs this statement also appeared in several of the "official" sketches that were published. As the story differed somewhat from the one that prevailed at Columbia when I was an undergraduate, I undertook to ascertain the facts for my own satisfaction and have arrived at the following conclusions:

In 1854 Wolcott Gibbs (easily the most distinguished of the many eminent scientists who have graduated from Columbia) was filling the chair of physics and chemistry in the College of the City of New York. He had only recently returned from Europe, where he had studied in Germany under Liebig and in France under Regnault, but had not as yet given any distinct evidence of his brilliant powers as an investigator, nor had he published papers that indicated his great genius.

It was also in that year that a successor was sought at Columbia for the illustrious James Renwick, who since 1820 had added to the prestige of his alma mater by serving her as professor of chemistry.

Various candidates were proposed and among them naturally enough the young alumnus of Columbia, who was then filling acceptably a teaching professorship in the Free Academy, as the City College was then commonly called. The trustees, however, in their wisdom chose Richard McCulloh, a man of more mature years than Gibbs and one who had already given promise of the future by his valuable work on the United States Coast Survey, then the foremost scientific bureau of the national government. That he filled the place satisfactorily is shown by the fact that three years later he was transferred to the chair of mechanics and physics, which he then held until October, 1863, when, as the General Catalogue has it, he "abandoned his post and joined the rebels."

Admirers of Professor Gibbs, however, have ever since persistently contended that Gibbs was rejected because he was a Unitarian, and even an appeal was presented to the New York state legislature[1] in which it was claimed that his rejection was made for sectarian reasons.

That Columbia has always had leanings toward the Protestant Episcopal faith is perhaps most significantly shown by the facts that the Bishop of New York and the rector of Trinity Church are ex-officio members of the board of trustees. But it must be remembered so also is the senior minister of the Dutch Reformed Church; and also again it must be remembered, that no evidence has ever been presented as to the faith of Professor McCulloh.

Much as I regret the decision of the trustees in depriving Columbia of the services of him, who, in the paths of science proved himself to be her most eminent alumnus, and also who ever inspired those who were so fortunate as to study under him with a true love of science, nevertheless, in these modern days, when church unity is the hope of so many, is it not time to cease the persistent criticism of Columbia for her sectarianism and to accept the more reasonable conclusion, entirely consistent with the facts, that McCulloh was chosen to the faculty because the trustees believed him to be the better man and not because Gibbs was a Unitarian.

Marcus Benjamin

THE LAW OF PRIORITY

On general principles it can not be denied

[1] Professor J. H. Van Amringe, Columbia's most beloved alumnus, in a recent letter, calls my attention to the fact that in response to this appeal a committee of the New York Senate was appointed to ascertain whether the trustees had required any "religious qualifications or test from any candidate as a condition of any professorship in said college." As a result of the inquiries the committee "arrived at the clear and decided conviction that there had been no such violation." See "A History of Columbia University. 1754–1904," New York, 1904, page 129.

that we must have uniform and consistent law, as has been stated by a recent contributor to the discussion, if we desire a stable system of nomenclature; in fact it goes without saying that this is quite essential.

But sundry knotty problems arise. For example when we observe in a recent catalogue that the word *Sunius,* for a well-known genus of beetles, which we have known hitherto only by that name, which our fathers and grandfathers knew only by that name, which in fact is the only name by which the genus has been known in virtually the entire domain of literature, must be changed and replaced by *Astenus,* we pause to ask why. It may be admitted that some one connected with the catalogue has gone back and at least thought he understood that the original diagnosis—these old descriptions being almost meaningless nine times out of ten—of *Astenus,* applied better to what we have known as *Sunius* than to anything else; but we are given no visible evidence whatever. Are we blindly to change the lifelong conception of several generations and reverse all published literature of the genus, on the authority of a guess and without presentation of any sort of proof? The language of the original description must alone afford this proof, for there is no way of knowing that the original type label may not have been shifted in some way, if the type chance to be in existence.

The pity of the interminable tangle may be reduced to this: If these over-zealous advocates of strict priority had only refrained from such publication until some system could be formulated, it would have been possible to adopt a uniform and consistent law which need not be necessarily that of rigid priority. One that might, for example, be analogous to the legal rule of exemption after a certain time limit. That is: If a genus name has not been challenged or corrected during a continuous period of say sixty or seventy years after its introduction in the commonly accepted sense, then it is to be considered permanent. This is absolute and consistent law and nothing else.

But the enthusiastic explorers of antiquity have spoiled this otherwise available recourse and I am free to confess that, as matters now stand, there seems to be no rational way out of the trouble but definitely to adopt the law of absolute priority. I would, however, only accept the identifications made by a competent commission, which should be compelled to publish its results in the fullest and broadest possible manner and in such a convincing way, by adducing the necessary proofs, that there could be no just ground for dissent. I feel that the enthusiasts aforesaid have compelled this course, because if we now use the old genus name *Ips,* for example, without further qualification, one would not know whether we refer to a Nitidulid or a Rhynchophorid beetle (*Tomicus* Latr.), to give only one instance among many.

So the very chaos which has come about through premature efforts to adhere to the law of strict priority now forces the adoption of that law, but only in the rigid way suggested above. In other words, incontrovertible evidence must be clearly and widely published, proving that the change is necessary. This opens up another vexing field of dispute. The subject is really serious and should be given the attention of the ablest natural historians now and without further delay, so that a secure foundation may be laid for future generations. Other work should be laid aside until this foundation is secure.

THOS. L. CASEY

WASHINGTON, D. C.

SCIENTIFIC BOOKS

Geometrical Optics. By ARCHIBALD STANLEY PERCIVAL. London, Longmans, Green, and Company. 1913. Pp. vi + 132.

This volume, issued recently, is intended for medical students as a text-book introducing them to so much of optical theory as may be necessary for the ophthalmic surgeon. The mathematics of the subject is hence free from applications of calculus, but the algebra involved is enough to cause most American

medical students to quail. The author assumes thorough knowledge of algebra, geometry and trigonometry, including particularly the vectorial significance of linear direction.

Physical optics is avoided entirely, since "no thorough elementary knowledge of that intricate subject can be obtained in the short time allotted to the student for studying optics." It is questionable whether this truth warrants the pedagogic loss involved in ignoring the wave theory of light. Elementary knowledge may be correct so far as it goes, but without involving intricacies. Children are taught in the grammar-school some of the conclusions resulting from the Newtonian theory of gravitation, but without any reference to the difficulties overcome in its establishment. The wave theory of light is now about as well established as the theory of gravitation. To assume it at the outset of a course in elementary optics is common enough to-day. For the college student this assumption is probably accompanied quite generally with the promise that he who perseveres will in time be provided with adequate foundation for the faith which is accepted without question at the outset. In deducing and applying the elementary formulas of optics the use of wave fronts is found to simplify demonstrations that are equally possible without them. Wave fronts and rays are quite inseparable instead of being mutually exclusive. The judicious teacher will be apt to guide himself by convenience and economy in reaching a decision as to a choice of methods of demonstration.

In text-books on optics there is unfortunately no definite consensus thus far in regard to the conventional assumptions to be applied in the development of theory. From the standpoint of the teacher and the manufacturer certain conventions may be useful which are unsatisfactory to the advanced student of theory. In every case they should be as simple as possible, so as to be really helpful. For the elementary student, and even the advanced student, probably the most troublesome snare is the minus sign. Mr. Percival says (p. 22): "We have adopted the usual conventions that directions from left to right are considered positive, and those from right to left negative." Similarly, upward is positive; downward, negative; counter-clockwise angular rotation is positive, clockwise, negative. This seems like simplicity itself; but in its application the elementary student of optics finds himself soon confused. In many cases mere magnitude is all that needs consideration, and to introduce additionally the element of direction, especially rotational direction, merely increases the chances of misinterpretation. For example, the deviation, D, which a prism of refracting angle A imposes on a beam of homogeneous light sent through it is commonly expressed in terms of A and the angles of incidence, ϕ, and emergence ψ, by the formula,

$$D = \phi + \psi - A.$$

Mr. Percival expresses this in words by saying (p. 43): "The total deviation is equal to the difference between the angles of emergence and incidence less the apical angle of the prism." A glance at the diagram is enough to satisfy any student of geometry that the former expression is correct. The author requests the reader to note that ϕ is measured clockwise and ψ counter-clockwise; but the introduction of this convention is here wholly unnecessary and misleading.

The formula for a thin lens in air is one of the most important in optics. Let us assume, as standard form, a bi-convex lens, with refractive index, n, radius of curvature r_1 on the side of incidence, and r_2 on that of emergence. Let this lens receive light from a radiant at distance u, and converge it to a conjugate focus at distance v. The relation existing is expressed by the equation,

$$\frac{1}{u} + \frac{1}{v} = (n-1)\left(\frac{1}{r_1} + \frac{1}{r_2}\right). \qquad (1)$$

The conventional assumptions involved are:

1. Irrespective of direction, the radius of curvature is positive for a convex lens surface, and negative for a concave lens surface.

2. Irrespective of direction, the curvature

is considered positive for a wave front propagated toward or from a real focus; and negative if from a virtual focus.

Another form commonly seen is,

$$\frac{1}{u} - \frac{1}{v} = (n-1)\left(\frac{1}{r_2} - \frac{1}{r_1}\right). \qquad (2)$$

The assumptions now involved are:

1. The direction from lens toward radiant is positive; its opposite is negative.

2. Curvature concave toward the radiant is positive; its opposite is negative.

If it is assumed additionally that the radiant is at the right of the lens, Mr. Percival's convention is expressed in Eq. (2).

The conventions connected with Eq. (1) have long been in common use. A converging lens is commonly called positive; a diverging lens, negative. Of late years Eq. (2) has been increasingly coming into use, for analytical reasons. The teacher of optics is free to take his choice; and this is apt to be influenced, in part at least, by ease of application. In a text-book published about twenty-five years ago by a pair of highly respected American college teachers of physics the deduction and discussion of Eq. (2) is given; but at its close they add the remark: "The equation is more simple in application if, instead of making the algebraic signs of the quantities depend on the direction of measurement they are made to depend on the form of the surfaces and the character of the foci." The conventions given in connection with Eq. (1) are then expressed. The present writer has tried both sets of conventions with his students; and with the result that pedagogically Eq. (1) is found much preferable. On examining thirty text-books in his library he finds Eq. (1) used in sixteen of them; Eq. (2) in thirteen; and both in one of them.

Mr. Percival seems to select the position of the radiant as origin, for in his diagrams he places this at the left, or negative, side of the lens or mirror; but this is not always done by him. He makes a distinction (p. 49) between the convention applied in *finding* a general formula and that applied in *using* a formula, saying, "when using the formulæ it will generally be found convenient to regard the direction of the incident light as the positive direction." The ordinary student, expecting uniformity and consistency, will be apt to stumble here, especially if he consults Edser's excellent book "Light for Students," and finds (p. 28), that "when the direction of measurement is opposite to that in which the incident light travels, the distance is positive." In this connection it should be noted that both Edser and Percival use the same form, expressed in Eq. (2). The positive direction for this equation may thus be either rightward, or leftward, or in the direction of propagation, or opposite to this direction, according to preference. The student probably has no preference, but wants definite information. After reversing his minus sign, and then re-reversing it a sufficient number of times, his mental condition becomes undesirable, to say the least.

Taking the equations as they are found in Mr. Percival's volume, he illustrates them by the solution of numerical problems, and in a number of cases additionally by graphic methods. The discussion of Gauss's cardinal points for a thick lens, or system of lenses, is perhaps scarcely full enough to enable the student to acquire very satisfactory working knowledge of the subject. Its application to the optics of the human eye is well illustrated both numerically and graphically.

An appendix is added in which a number of topics of practical importance are treated mathematically, without any attempt to avoid or disguise the notation of calculus. Medical students, for the most part, may naturally be disposed to accept the results without mastering the details of demonstration.

There are a few obvious typographical errors that will probably be corrected in a future edition. Despite the uncertainties about linear and angular direction, the book is clearly written, and by one who has evidently had good experience in dealing with students. It is worthy of commendation to those for whom it was intended.

W. LECONTE STEVENS

LEXINGTON, VA.,
September 2, 1913

Prevention and Control of Disease. By Francis Ramaley and Clay E. Griffin. Copyright by Francis Ramaley, Boulder, Colorado. 1913.

In the preface to the book the authors state the purpose for which it has been written. The work of investigators, physicians and public health officers should be more widely known in order that an intelligent body of citizens may cooperate in its extension. The book is intended for the general public and as a text for college classes. It is not written for medical students or biologists. After discussion of death rate, types of disease and certain hygienic considerations nine chapters, constituting almost half of the book, are given to a concise summary of the "germ theory of disease," the nature, life-history, metabolic activity and distribution of animal and vegetable parasites, the mode of infection and spread of infectious diseases, disinfection, susceptibility and resistance, immunity and specifics in the treatment of disease. One familiar with the complexity of any biological science may doubt the possibility of conveying to the general reader a conception of the nature of the objects or of the phenomena described or in the absence of a clear understanding of the subject the possibility of maintaining his interest. For those who wish this information a satisfactory synopsis is furnished. It is even more doubtful if matter described in this part of the book can be used as the basis of a collegiate course. To appreciate the form and life-history of bacteria and protozoa and the chemical changes caused by them both preliminary biological training and objective demonstration of selected forms may be regarded as essential. Study of the phenomena of immunity including the intricacies of Ehrlich's side-chain theory or of phagocytosis and opsonic action must be relegated to the biological student who wishes to acquire technical training and superficial information may leave the impression of occult mystery in the mind of the general reader. The book contains a large amount of information which the layman should have and it is presented in interesting form. The statements concerning medical practise are generally accurate, but occasionally an indefinite or erroneous impression is produced. Advice to eat moderately at the beginning of a "cold" may be worth heeding, but its value is not strengthened by the suggestion that side-chain receptors become coupled to toxins when intoxication takes place and the body is unable to assimilate food until new side chains are developed. The cause, dissemination and prevention by personal and governmental precautions of "cold," diphtheria, contagious diseases of childhood, tuberculosis and other diseases are adequately discussed. The value of vaccination and of the serum treatment of diphtheria is emphasized with the purpose of overcoming lingering prejudice. As an illustration of desirable information which may aid the layman to judge his professional attendant may be cited the author's discussion of the importance of surgical cleanliness on the part of dentists. Historical data defining the changes that have occurred in the prevalence of certain diseases or describing the progress of medical discovery add interest and clearness to the book.

E. L. Opie

SPECIAL ARTICLES

ON INDUCING DEVELOPMENT IN THE SEA-URCHIN (ARBACIA PUNCTULATA), TOGETHER WITH CONSIDERATIONS ON THE INITIATORY EFFECT OF FERTILIZATION[1]

I. THE INITIATION OF DEVELOPMENT WITH DILUTE SEA WATER

In the course of work on the energetics of development, it became necessary to study in detail the question of water absorption at various stages of embryogeny. For certain phases of these studies the eggs of *Arbacia punctulata* proved extremely favorable. In various concentrations of sea water these eggs behave exactly as expected, but in 25 per cent. sea water (25 c.c. sea water + 75 c.c. H_2O dist.) fertilization membranes appear. The process takes place in from one to one and a half minutes at ordinary temperatures. In two minutes many eggs as well as their nuclei

[1] Preliminary communication.

are cytolized, and in three minutes this is true of most of the eggs.

The membrane in question is a true fertilization membrane, and if at the proper moment the eggs are brought back into normal sea water, or better still, hypertonic sea water (50 c.c. sea water + 8 c.c. 2.5 N NaCl), cleavage takes place. Since July 18 I have succeeded in rearing a considerable number of ciliated larvæ.

II. THE INITIATION OF DEVELOPMENT WITH EGG EXTRACT

If fresh ovaries of *Arbacia* are ground up in a mortar with pulverized glass and a small quantity of sea water, the liquid, when filtered, has a color not unlike that of blood serum. This fluid, if allowed to act on ripe eggs contained in an equal quantity of sea water, proves to be an excellent initiatory agent, for if the eggs after one to two hours are placed in normal sea water, many divide, although no fertilization membrane appears.

III. THE THEORY OF INITIATION, PARTHENOGENETIC METHODS AND THE FERTILIZATION MEMBRANE

It is well known that development can be induced in many kinds of eggs by very diverse means—lipoid solvents, increased osmotic pressure of the surrounding medium, electricity, heat, cold, mechanical shock and even pricking the egg surface, have all proved effective in one case or another, but so far as I am aware the use of egg extract from the same species is new, as well as the production of genuine fertilization membranes in *Arbacia punctulata* by means of dilute sea water. In one of the California sea urchins, Loeb[2] has reported the formation of membranes after the addition of distilled water, but from certain details it seems that the fertilization membrane of at least one of the California urchins resembles that of *Asterias forbesii*, and this differs quite markedly from that of *Arbacia punctulata*.

Loeb,[2] on the basis of his own investigations and those of others, has formulated a theory on the initiation of development which for normal fertilization and certain of the parthenogenetic methods, postulates (*a*) an increased permeability of the ovum due to the action of lipoid solvents or hæmolytic agents; (*b*) the formation of a fertilization membrane in consequence of this superficial cytolysis.

Of an increase in permeability synchronous with the initiation of development there is not the slightest doubt, although the great variety of parthenogenetic methods long ago indicated that permeability is increased, in other ways than by action on surface lipoids. With the employment of some parthenogenetic methods, fertilization membranes appear, with others, not, and even the employment of lipoid solvents themselves may or may not be followed by the appearance of a fertilization membrane. One and the same egg, as in the present case, may be induced to develop with or without the appearance of such a membrane.

IV. EXPERIMENTAL ANALYSIS OF THE FERTILIZATION MEMBRANE

According to Kite's[3] dissection, the egg of *Arbacia* has a vitelline membrane tightly glued to its surface. Outside this is a thin jelly. The appearance of the fertilization membrane, according to this description, is due to the swelling of the vitelline membrane, and the formation of a phase boundary between it and the thin outer jelly.

This description I believe to be essentially correct for the following reasons:

1. The fertilization membrane also has an inner visible boundary. In certain localities of the two- and four-cell stage this inner surface of the fertilization membrane is plainly visible, has indeed been often figured and I believe misinterpreted. In the stages in question a narrow perivitelline space can be seen around the egg, but the fertilization membrane adheres to the egg surface here and there by strands. As a consequence, when

[2] Loeb, Jacques, "Die chemische Entwicklungserregung, etc.," Julius Springer, Berlin, 1909.

[3] Kite, G. L., "The Nature of the Fertilization Membrane, etc.," SCIENCE, Vol. XXXVI.

the egg divides, some of these strands are drawn down between the cleavage cells, and as certain portions of the surface of these are further removed from the fertilization membrane than the original egg, the inner limit of this membrane, as well as the perivitelline space itself, becomes visible. The perivitelline space seems to be identical with the so-called "hyaline plasma-layer," and homologous with the perivitelline space of the fertilized starfish egg.

2. By means of hypertonic solutions as well as by extract of themselves, sea-urchin eggs can be induced to divide without the appearance of a fertilization membrane. Development, however, does not proceed normally because the blastomeres fall apart. Since the vitelline membrane is tightly glued to the surface of this egg and a perivitelline space appears after the membrane has swollen, eggs dividing without the formation of this space have the membrane adhering to the resulting blastomeres. In consequence, these cells, instead of being in intimate contact with one another as they normally are, are each enclosed in a separate vitelline membrane. In other words, when the vitelline membrane is not lifted off the egg surface, it divides with the egg, which is what one would expect. If this idea is correct, cleavage cells which have originated by the division of an egg without a "fertilization" membrane should be able to "form" such membranes under suitable conditions, and this I have observed. Immersed in dilute sea water, isolated cleavage cells, derived from ova which have not formed "fertilization" membranes, form them in from one to two minutes.

3. Egg fragments can also be produced by shaking. No fertilization membranes appear in such eggs or their fragments as the result of the mechanical agitation, but when treated with dilute sea water or sperm, membranes appear in some of the fragments, but not in others. Both kinds of fragments have been fertilized with sperm and allowed to develop, some with and some without the membrane. This result can only be understood if we accept Kite's discovery that the fertilization membrane in *Arbacia punctulata* appears when a preexisting jelly, closely adherent to the surface of the egg, swells and changes its optical properties.

4. From the above experiments one may infer that a fertilization membrane may appear around part of an egg, instead of the whole. If Kite's jelly is ruptured the egg flows partially through the hole in the membrane, and assumes a dumbbell shape. If it is now fertilized with sperm, or treated with dilute sea water, a fertilization membrane appears on one sphere of the dumbbell, but not on the other. Such eggs are capable of development.

5. The appearance of a fertilization membrane in *Arbacia punctulata* is not a function of the living egg, for if the egg is crushed, or even dried completely in a desiccator for days, membranes still appear after proper treatment.

V. WHAT MAKES THE FERTILIZATION MEMBRANE APPEAR NORMALLY?

If the interpretation given to the results outlined is correct for *Arbacia punctulata*, it is easy to see why the fertilization membrane should appear in dilute sea water, or in distilled water. But why does it appear under normal conditions in sea water?

The exact mechanism of the process is not yet clear, but it seems to be a function of the number of sperm present. If one inseminates eggs very carefully so that not more than four or five spermatozoa come into contact with each one, the fertilization membrane does not appear. I have repeated this experiment many times and have controlled it by the most careful observations with different powers on fresh material as well as stained. Such preparations show sperm plainly adhering to Kite's jelly in every egg, but the "membrane" does not appear. Eggs treated in this manner do not develop, although some of the smaller ones may form asters. What it is in the sperm that brings about the swelling of the jelly has not yet been determined. However, beautiful fertilization membranes may be caused to appear in two to three hours by treating the eggs with minute infusoria.

No membranes appear in the controls, nor do the eggs whose membranes have appeared develop when returned to sea water. Three possibilities suggest themselves—an acid effect, a mechanical effect or a heat effect. No decisive experiment has as yet been devised.

These experiments suggest that in *Arbacia punctulata* the membrane swells before the sperm enters the egg, and not after. Experiments also show that when the phase boundary between Kite's jelly and the outer jelly is complete, sperm do not readily penetrate the fertilization membrane. From this it follows that the penetration occurs at the moment when the jelly is softened and begins to swell. Accordingly, eggs whose jelly has been partially softened by heat or infusoria should be capable of fertilization with small doses of sperm. This has actually been observed in a number of instances. The opposite experiment of hardening the jelly with Ca has been performed. Such eggs are extremely difficult and in many cases impossible to fertilize as the sperm do not stick.

VI. THE RELATION BETWEEN FERTILIZATION AND THE FERTILIZATION MEMBRANE

The relation between the initiation of development and the fertilization membrane in *Arbacia punctulata* is one of association rather than "causal," for the membrane may be made to appear without development, and development may be initiated without the appearance of the membrane. In *Asterias forbesii* the association is somewhat different, and so intimate that any method which causes the membrane to appear is at the same time a method of initiation provided the violence is not too great and the egg is in good condition and in a suitable medium. The explanation is simple. In *Asterias* the fertilization membrane does not depend on the swelling of a formed jelly, but instead, the egg peels itself away from the inner surface of a thin pre-existing membrane. This peeling away seems to depend, not upon changes in the fertilization membrane, but upon changes in the surface film of the egg. When this is rendered more permeable, material leaves the egg and the egg shrinks away from its closely adherent covering which thus becomes visible. The perivitelline space in the starfish egg is homologous with that of the sea urchin egg, but is much larger.

The type of fertilization membrane found in *Arbacia punctulata* may be called hydrophilous, that of the starfish, *Asterias forbesii*, anhydrophilous.

VII. ON THE LOSS OF SUBSTANCES BY THE EGG AND THEIR NATURE

The starfish egg upon peeling off from its anhydrophilous fertilization membrane is markedly smaller in volume than before. The same thing is true of *Arbacia*. Exact measurements will be given when I publish exact details of these experiments. No doubt much of the material lost by the egg is water. F. R. Lillie[4] in a series of fundamental researches has shown that the fluid over-fertilized eggs may contain at the least two classes of substances, (*a*) "iso-agglutinins" and (*b*) a substance having a chemotactic influence on the sperm. From Elder's[5] investigations as well as certain observations of my own, it appears possible that the chemotactic substance is contained in the outer jelly of the *Arbacia* egg. I have been able to verify the "iso-agglutinin" and its effects as described by Lillie in the case of *Arbacia* and *Asterias*.

Ovarian extract of *Arbacia*, when present in sufficient quantities, retards the development of normally fertilized *Arbacia* eggs. If the extract is added to blastulæ which have developed in normal sea water, these are instantly slowed down and absorb water. *Arenicola* larvæ also have their permeability increased by the *Arbacia* extract, as can be very prettily seen by their loss of pigment. They also slow down in their movements and are slightly and reversibly agglutinated.

[4] Lillie, F. R., "Studies of Fertilization," I. and II., *Jour. of Morph.*, Vol. 22; III. and IV., *Jour. of Exp. Zool.*, Vol. 12; V., *Jour. of Exp. Zool.*, Vol. 14.

[5] Elder, J. C., "The Relation of the Zona Pellucida to the Formation of the Fertilization Membrane," *Arch. f. Entwicklungsmechanik*, Vol. 36.

These observations suggest that the ovarian extract, as well as the secretions of the egg on fertilization contain substances that not only influence permeability, but may reduce the oxidations in the cell.

VIII. THE THEORY OF INITIATION

The theory of initiation, as given by Loeb, postulates essentially that initiatory influences place the egg in a condition in which its oxidative processes can proceed, or proceed normally. This is accomplished by increasing the permeability of the egg, and in the case of many parthenogenetic agents, as well as in normal fertilization by sperm, the permeability change may be brought about by lipoid solvents. The fertilization membrane may or may not appear after the use of lipoid solvents, and when, as in the case of the starfish egg, it does appear, it may also be made to do so with any other method of increasing the permeability of the plasma film. These facts, many of which have been emphasized by Loeb,[5] R. S. Lillie[6] and others, by no means prove that the theory of initiation is wrong. Indeed, they are all in harmony with this view if we remember that an hydrophilous fertilization membrane may or may not appear, depending on circumstances, whereas an anhydrophilous one like that of *Asterias* is certain to appear when, as the result of a permeability change, the egg shrinks away from its enclosing capsule.

How can increased permeability initiate development?

The ovum demonstrably has the necessary mechanism to undergo development of itself. It is a cell with a long metabolic history and before development is initiated its plasma film is relatively impermeable. This may involve the accumulation of "waste" products, and these we may believe to automatically inhibit further metabolic processes. Loeb has shown that these processes are oxidations, and my experiments show that substances can be extracted from the eggs which reduce the rate of development and have a marked effect in decreasing the activity of *Arbacia* as well as *Arenicola* larvæ. It does not seem unreasonable to suppose therefore that these materials are active because they reduce oxidations. The mere fact that they also increase cell permeability and are good initiatory agents is beside the point, for increased permeability in *Arenicola* larvæ is also associated with acceleration of movement.

One may extend the theory of initiation and assume that all agencies that initiate development do so because through increased permeability of the plasma film the egg is enabled to loose substances antagonistic to oxidation. By freeing itself of these inhibitors, a chemical equilibrium is disturbed, and oxidation, and with it development, is free to go on.

In this way we can explain why a mature starfish egg, if unfertilized, may oxidize itself to death, for we may suppose that its permeability has been sufficiently increased by maturation to accelerate oxidation, but not enough to initiate development proper. We can also bring all parthenogenetic methods whatsoever, as well as normal fertilization, under a common point of view, for the increased permeability, no matter whether produced by electricity, heat, cold, mechanical shock, specific chemical alteration of the membrane, lipoid solvents, or pricking, is all that is necessary to enable the egg to free itself from its accumulated inhibitors. Why the egg should develop after treatment with hypertonic solutions is also clear, for if in such media the plasma film is permeable to the inhibitors, loss of water by the egg would, directly or indirectly, accelerate the loss of antagonists. That these are lost in hypertonic sea water is shown by special experiments.

In conclusion, I must thank my colleague, Dr. W. E. Garrey, who kindly allowed me to demonstrate to him various steps in the investigation, and to whom I am indebted for a number of valuable suggestions and criticisms.

Otto Glaser

Woods Hole, Mass.,
August 4, 1913

[6] Lillie, R. S., "The Physiology of Cell Division," *Jour. of Morph.*, Vol. 22.

THE SOCIETY OF AMERICAN BACTERIOLOGISTS. III

PATHOLOGIC BACTERIOLOGY

Cultivation and Differentiation of Fusiform Bacilli: CHARLES KRUMWIEDE, Jr., and JOSEPHINE PRATT, Research Laboratory, Department of Health, New York City.

Isolation: Dilutions of the original material are made in a series of tubes of ascitic fluid or horse serum. To these is added fluid agar and they are then poured in the covers of petri dishes. While the agar is still fluid the lower part of the petri dish is laid on the agar, giving a layer of agar between the two parts of the dish. After forty-eight to seventy-two hours the upper glass is separated from the agar and the distinctive colonies fished. The colony is characterized by the thread-like outgrowths from one or both sides of the colony. Cultivation: A semi-solid medium employing stab inoculation is most convenient for preservation of cultures. The puncture closes after inoculation and subinoculations are easily performed, due to the softness of the medium. Aerobic contamination is quickly noted. The medium is prepared as follows:

Agar	10 gms.	2 parts
Gelatin	80 gms.	
Veal broth, 2 per cent. peptone, no salt	1,000 c.c.	
Horse serum or ascitic fluid		1 part

Horse serum has given more uniform results than ascitic fluid. Although there is a difference in various strains in their ability to grow on simple media, serum containing media are necessary for surety of cultivation of all strains.

Source and Number of Cultures being Studied

Noma	2 strains	Total 15
Vincent's angina	5 strains	
Spongy bleeding gums	1 strain	
Pyrrhœa	2 strains	
Chronic otitic media foul discharge	3 strains	
Carious teeth	1 strain	
Ulceration of tongue	1 strain	

Morphology and Cultural Differentiation: The typical bacillus is more or less pointed. In cultures they are extremely pleomorphic, filaments and wavy forms simulating spirochetes being found. No morphological differentiation has been made. Sugar fermentations show some differences, but these differences show no relation to the source of the culture. Pathogenicity: Abscesses can be produced under the thin skin covering the cartilage of the ear of rabbits. Relation to Spirochetes: Spirochetal-like forms can be found especially in fluid media. The relation of these to the spirochetes in the original material has not been sufficiently studied.

The Morphology of Cultural Amebas: ANNA WESSELS WILLIAMS, Research Laboratory, Health Department, City of New York.

The paper was a report of the studies on cultural amebas grown under conditions as nearly as possible like those of the habitat from which the amebas were obtained. "Ameba 11,524," obtained originally by Musgrave and Clegg from the stools of a case of human amebic dysentery, when grown on fresh brain tissue medium at high temperature (34° C. to 38° C.) for several days with the addition each day of fresh blood and, after two days, of small amounts of certain bacteria, continues a vigorous growth and shows from day to day a marked pleomorphism. The organisms lose their contractile vacuole and the nuclei assume many of the appearances described as characteristics of the "entameba" group in man. As many as eight nuclei have been found in a trophozoite, and six in a cyst, the usual number so far seen is four in each. In this particular as well as in size and in a "cyclic" change of the karyosome, this species most frequently resembles the pathogenic species described as *Entameba tetragena.* Conclusion: (1) Cultural amebas isolated from the stools of dysenteric patients, are much more complicated in morphology than we have been led to think, and grown under conditions approaching those found in the intestines they closely resemble species described as strict parasites. (2) The question of species and pathogenicity of amebas found in dysenteric stools will probably be settled finally and not until then, when a comparative study is made of amebas in their natural habitat with pure cultures isolated from the same cases and grown *under conditions similar to those found in the habitat from which many were isolated.*

Observation on the Intestinal Bacteria in Pellagra: W. J. MACNEAL.

This report is based upon the work of the Illinois State Pellagra Commission[1] and of the Thompson-McFadden Pellagra Commission of the New York Postgraduate Medical School. A general survey of the fecal bacteria by the methods previously employed in studying the feces of healthy men[2] showed considerable variation from

[1] *Archives of Internal Med.,* August, 1912, pp. 123–168, and September, 1912, pp. 219–249.

[2] *Journ. of Infec. Diseases,* Vol. 6, No. 2, April, 1909, pp. 123–169, and Vol. 6, No. 5, November, 1909, pp. 571–609.

the normal numerical relationships and the advent of new types of bacteria, not observed in healthy men. The most evident change was the relative increase in certain normal types such as *B. bifidus*, *B. welchii* and the micrococci. The cocci were always increased during the acute attack. Other changes were not constant. About 800 bacterial strains were isolated by plate cultures of feces and of intestinal juice obtained through the Einhorn duodenal tube, and these were subjected to agglutination tests with serum from pellagrins at Peoria, Kankakee and Chicago, Ill., and Spartanburg, S. C. One of the bacterial strains is completely agglutinated by the serum of 81 of 109 cases of pellagra (74.3 per cent.), and by 11 of 45 control cases believed to be free from the disease. Similar organisms have been found in the duodenal juice in a few others. The work is being continued.

A Study of Diarrhea in Infants: A. W. Street, Brown University.

This work is a study of the rapid diagnosis of dysentery from the stools of infected infants. It consists of inoculation in special broth tubes from the swabs of the stools, and subsequent isolation of the organism believed to be the cause of diarrhea. We used litmus-lactose-agar and Endo plates and transferred the characteristic growth to other tubes to show cultural characters. In our work we found Russel's medium particularly good for differentiation of the group, and litmus-milk good for differentiation of the two main types. We also used lactose-peptone bile, saccharose and dextrose broth, gelatine, peptone and mannite-litmus semi-solid medium. The incubations were all at 37° C. except gelatine, which was at 20° C., but for varying lengths of time. Generally the incubations were for eighteen hours. Not all the cases were sent to be diagnosed—only the most severe and those reported to the nurses by the physicians. The agglutination test, which is recognized as the most conclusive, was not regularly tried, because of the fact that no good serum was immediately procurable. Agglutination occurs, however, in dilutions of 1:200 and 1:500. Of the cases sent in, which numbered 47, seven showed reactions of those of the dysentery group. They produced acid in litmus milk, and so are of the Flexner type. Many showed reactions in culture tubes very similar to the control tubes, but these failed to check up in one tube or another. So that we are able to conclude that the method of rapid isolation is practical, as is shown in seven of forty-seven cases, or 14.89 per cent.

Bacteriology and Control of Acute Infections in Laboratory Animals: N. S. Ferry, Detroit, Michigan.

Diseases in Epidemic Form Studied: An infection in rabbits, dogs, guinea-pigs and monkeys due to the *B. bronchisepticus*, the microorganism which has been found to be the cause of canine distemper and an infection among rabbits due to the bacillus of rabbit septicemia. Study of Organisms Resembling the *B. bronchisepticus:* During the course of the studies on the epidemic which raged among the several animals ten different organisms were encountered which resembled the *B. bronchisepticus* somewhat in their morphology, their early growth on agar and their behavior toward Gram's stain. A careful study of these organisms showed them to be connected with the epidemic only in the capacity of secondary invaders. Primary Infection: The primary infection was found to be due to the *B. bronchisepticus*. Agglutination tests showed the *B. bronchisepticus* to be absolutely distinct from any of these other organisms. Control of Epidemics: The epidemics were controlled by isolation, antisepsis and the use of prophylactic injection of vaccines made from the specific microorganisms. Epidemic due to the Bacillus of Rabbit Septicemia: After the epidemic due to the *B. bronchisepticus* was under control, an epidemic broke out among the rabbits, due to the bacillus of rabbit septicemia. This epidemic proved very fatal before it could be controlled. The same methods of control were carried out as before. Value of the Protective Inoculation: Although all animals can not be saved by means of the prophylactic injection, control experiments have proved that a large majority are protected.

The Lesions produced by Intra-bronchial Insufflation of B. prodigiosus: Martha Wollstein, M.D., and S. J. Meltzer, M.D.

We inoculated broth cultures of *B. prodigiosus* into the lungs by means of intra-bronchial insufflation, which consists of the introduction of a tube through the mouth, larynx and trachea into a bronchus, and the injection of the fluid culture through the tube. Doses of 5 c.c. to 15 c.c. of a twenty-four-hour broth culture injected into the lungs of dogs were uniformly fatal in six hours to three days, the great majority of animals dying within twenty-four hours. It was not until the dose was reduced to one cubic centimeter that three out of five dogs survived until the fourth day. The entrance of *B. prodigiosus* into the blood stream followed intra-bronchial insufflation

of all doses of one cubic centimeter or more of this organism. The bacillus grew profusely from the heart's blood of every case examined, from five and three fourths hours to four days after inoculation. After the fourth day no growth could be obtained from either heart or lungs. The pulmonary lesions produced by intra-bronchial insufflation of *B. prodigiosus* in dogs differed very markedly from the experimental pneumonias which have hitherto been produced by other bacteria administered in this way. Thus large doses (5 to 15 c.c.) caused pulmonary lesions which were chiefly hemorrhagic and necrotic in character, with a large production of fibrin. Bacteremia and death were the rule. The lung lesions were more severe and the death rate higher than was the case with other bacteria administered intra-bronchially. Very small doses (0.5 c.c.) caused a fibrinous inflammation, lobular at first, later coalesced and lobar in distribution, without evidence of necrosis. No bacteremia followed these small doses and recovery was possible.

Frequency of Vincent's Angina among Routine Throat Cultures: JOHN L. RICE, Syracuse Medical School.

From the examination of 1,352 routine throat swabs 10, or seven tenths of one per cent., were found to be both bacteriologically and clinically Vincent's angina, both the fusiform bacillus and the spirochete being found. Four of the ten cases were clinical cases of diphtheria, showing that 40 per cent. of the Vincent's angina cases were also positive for diphtheria. In twelve other cases of the 1,352 the bacilli or the spirochetes were found alone. None of these twelve cases were clinically Vincent's angina. Morphologically the bacilli in the clinical and non-clinical cases were alike. In a microscopical examination of a smear from the swabs a diagnosis can be made by finding fusiform bacilli and spirochetes in symbiosis, even though the number of spirochetes present may be small.

Studies on the Etiology of Hog Cholera: WALTER E. KING and F. W. BAESLACK, Research Laboratory, Parke, Davis & Co., Detroit, Mich.

This report is presented for the purpose of recording certain observations, which have been made by the aid of the dark field on the blood of hogs suffering from hog cholera. During the last few months a spirochete has been found with uniformity and constancy in the blood of every cholera hog examined. Practically all of these positive findings have been controlled by one or more careful dark field examinations of the blood before inoculation. Additional checks are furnished in several cases by negative findings subsequent to positive results in blood of hogs recovered from the disease. In so far as the present results go, the practised observer can readily distinguish certain characters in the blood of animals suffering from hog cholera when placed on the dark field, as differentiated from normal hog blood. Hog cholera blood usually contains many granules, some very fine yet distinctly larger than blood dust, some larger still, and some very distinct, highly refractive bodies. It is possible that some or all of these granules represent disintegrated blood elements resulting from the disease. It is suggested, however, that some of these granules may represent certain stages in the life cycle of the spirochete under observation. To date, positive findings of this spirochete are recorded in 10 strains of virus from the blood of 33 hogs suffering from the disease. Controls are furnished by negative findings in the blood of about 50 normal hogs and in the blood of six animals which became convalescent and finally recovered. Two experiments have been made relative to the horse serum virus phenomenon, which showed the presence of the spirochete in the horse serum virus.

IMMUNITY BACTERIOLOGY

The Relation of the Leucocytic Bacteriolysin to Body Fluids: W. H. MANWARING, Rockefeller Institute for Medical Research.

A bactericidal substance can be extracted from horse leucocytes. This substance is strongly bacteriolytic when dissolved in distilled water and possesses considerable bactericidal power when dissolved in physiological saline. The substance, however, is without bactericidal properties when mixed with sera, with pathological transudates, with cerebro-spinal fluid, or with the products of tissue autolysis, including the products obtained by a prolonged autolysis of leucocytes themselves. The antibactericidal action of body fluids and tissue products depends upon three factors: (1) the antibactericidal power of the colloids they contain, (2) the antibactericidal power of their neutral salts and other neutral diffusible components and (3) the antibactericidal power of their diffusible alkalies. Diffusible acids are apparently without antibactericidal effect. An extract from horse leucocytes can have little or no antiseptic action, when injected into body cavities and tissue spaces.

On Intraperitoneal Lysis of Tubercle Bacilli: W. H. MANWARING and J. BRONFENBRENNER, Rockefeller Institute for Medical Research.

If suspensions of tubercle bacilli are injected into the peritoneal cavities of tuberculous guinea-pigs, there takes place a rapid disappearance of the bacilli from the peritoneal fluids, as determined by subsequent examinations by the Ziehl-Neelson method. Nine tenths of the bacilli may disappear within an hour, and all but an occasional bacillus within five hours. This disappearance is paralleled by the appearance of atypical, non-staining and granular forms. After the disappearance numerous granules can be demonstrated in the peritoneal fluids and peritoneal scrapings by the Much method. Before the conclusion can be drawn, however, that the disappearance of the tubercle bacilli is due wholly to their destruction by the peritoneal fluids, such factors as a possible removal of the bacilli by the rapid formation and absorption of peritoneal transudate must be ruled out, as well as the possibility of a spontaneous metamorphosis of the bacilli into non-staining and therefore invisible forms, as described by Much. A similar rapid disappearance is brought about in the peritoneal cavities of tuberculous rats, tuberculous rabbits and tuberculous dogs. The mechanism of the disappearance is now under investigation.

The Chemistry of Anaphylactic Intoxication: Benjamin White, Hoagland Laboratory, Brooklyn, N. Y.

The study of the chemical problems involved in the anaphylactic phenomenon would seem to offer a promising field. If this reaction is to be considered as a parenteral digestion of protein, then it may be possible to study the reaction *in vitro*. The work of Vaughan on the poisonous substance obtained from proteins by alkali hydrolysis, the work of Biedl and Kraus and others on the action of proteoses and peptones and the experiments of Rosenow on the products of *Pneumococcus* autolysis, appear to be closely related, and these in turn bear resemblances to the results of experiments on the anaphylatoxin produced in the test tube. Recent studies on the action of the amines, particularly that of B-amid-azolylethylamine, suggests a possible analogy between the action of this class of substances and the substances mentioned above.

Peptotoxin Production by the Bacillus of Contagious Abortion in Cattle:[1] John Reichel, V.M.D., and Malcolm J. Harkins, V.M.D.

The English commission appointed by the Board of Agriculture and Fisheries to inquire into epizootic abortion of cattle, in their report include the statement "apparently, however, no free toxins are formed by the bacillus (abortus) in culture." The reaction in infected cattle, usually appreciable by a rise of temperature, etc., in from eight to eighteen hours after a subcutaneous injection of abortin, *i. e.*, an extract of the bacillus and its products prepared as in tuberculin with tubercle bacilli is generally attributed to toxins of which the English commission remarks, "the toxins, then, which cause the febrile symptoms after inoculation are endotoxins, that is to say, they are contained inside the bacilli." From this it may be taken that the opinion is held that the bacilli in culture form no other toxins than endotoxins. From our experiments we have drawn the following conclusions: (1) The bacillus of contagious abortion of cattle (*abortus bacilli*) produces a toxin on peptonized culture media, but not on peptone-free media. (2) Thorough washing will rid the bacilli grown on peptonized media of the toxin. (3) The toxin is included in the alcoholic precipitate of the supernatant liquid of the suspension of the bacilli grown on peptonized agar. (4) Sixty-five degrees centigrade for thirty minutes apparently had no effect on the peptotoxin. (5) Cattle must be sensitized to react to the peptotoxin. (6) *Bacillus typhosus, coli communis,* tetanus and pneumococcus cultures on peptonized agar reveal the presence of peptotoxin, when injected into animals sensitized to the abortus bacillus or its products. The peptotoxins of these organisms probably have much in common if they are not one and the same substance, because animals can be sensitized with one for any of the others. (7) No reactions were observed following the injections into the sensitized animals of peptonized agar cultures of the diphtheria bacillus, *Staphylococcus aureus,* nonhemolytic streptococcus and hemolytic streptococcus which may mean that these organisms did not produce peptotoxin or only in very small amounts. (8) Rabbits developed agglutinins following the injection of thoroughly washed and unwashed abortus bacilli equally well. The peptotoxin injected with the unwashed bacilli is not essential in the production of antibodies. (9) In that the abortus bacillus produces a peptotoxin in a proteid medium—and it is a possibility that the peptotoxin is produced in milk with the bacilli from cattle in infected herds—the wholesomeness of such milk is more than questionable.

[1] The Mulford Laboratories, Glenolden, Pa.

A. Parker Hitchens,
Secretary